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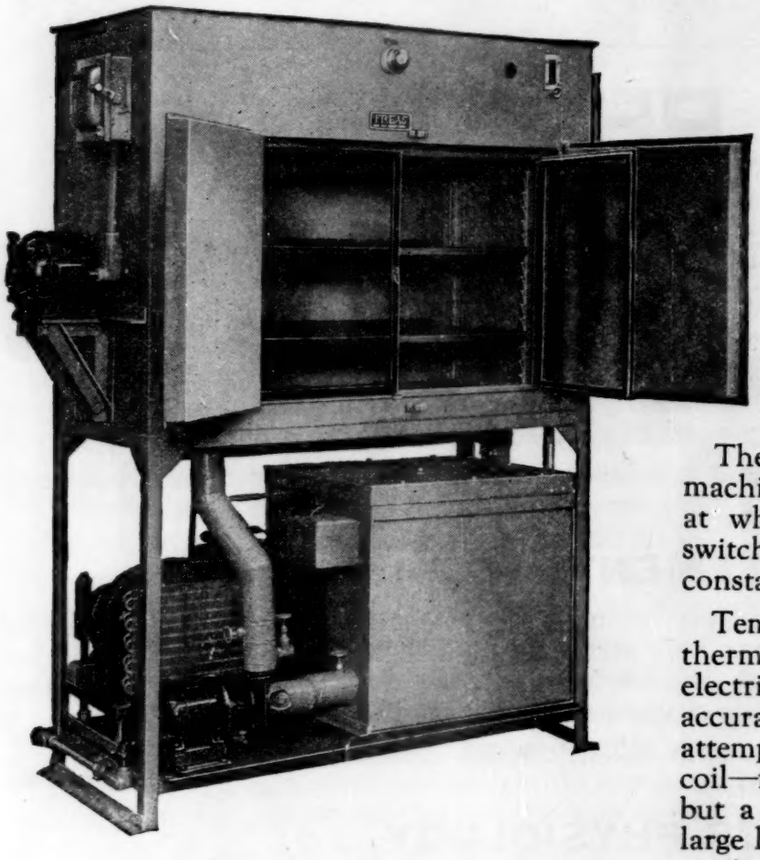

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REALITY IN PHYSICS[1]

By Dr. W. F. G. SWANN

DIRECTOR OF THE BARTOL RESEARCH FOUNDATION OF THE FRANKLIN INSTITUTE

I SUPPOSE there are few things concerning which one could find so many differences of opinion as the question of what constitutes a theory in physical science. The pure mathematician would probably be content with a procedure of the following kind. He will set up a branch of mathematics founded upon certain postulates having to do with quantities, letters, etc., that he chooses to be talking about. In this mathematical scheme, there will appear relationships between certain quantities which occur in the mathematics, and it will be his hope to invent a scheme of mathematics of this kind which shall form an analogue of the regularities of nature in the sense that there may be a one-to-one correspondence between certain things in the mathematics and the observable phenomena in nature. It has been said that the pure mathematician is never as happy as when he does not know what he is talking about; and, in the foregoing method, he establishes contacts with the physical world with a minimum degree of shock to his own conscience, by a procedure in which he ceases to think of anything physical until he has completed his mathematical structure, and confines all physical contamination of that structure to the act of setting up the correspondence to which I have already referred. When the correspondence has been set up, the postulates of his mathematics become the laws of nature in the physics. It is possible that he may choose his postulates in various ways. In the journeys which he takes from his postulates as starting points, he arrives at multitudes of conclusions. He might gather together a suitable collection of these at any stage and

[1] Address of the president of the American Physical Society given at New Orleans on December 30, 1931.

take them as his starting point, and continue his reasoning backwards and forwards over the whole realm of logical regularity which he traversed before. In his journey through the realm of the abstract mathematical thought of his theory, he may stop or start at various stations. Where will he prefer to start? He will probably prefer to start at the place where the number of his postulates is a minimum. To him, a good theory will be one in which by saying few things he can deduce, as their consequence, many things. The value of his theory to the physicist will lie in the fact that it not only correlates the phenomena which he, the physicist, has discovered, but suggests multitudes of other phenomena which he may look for, and also suggests ways of viewing the interrelationships between the phenomena already studied other than those which he has already adopted. The physicist may have taken an intellectual journey from the set of phenomena "A" to the set of phenomena "B," analogous to my taking a journey from Philadelphia to Washington via the Rocky Mountains, California and New Orleans, and the complete mathematical structure may show that shorter journeys, analogous to my going from Philadelphia to Washington through Baltimore could be made.

The postulates which are agreeable to the pure mathematician will not necessarily be agreeable to the physicist, because the latter thinks that he knows what he is talking about and glories in the fact. It will be little consolation to the physicist to know that by the assumption of few things he may deduce many, provided that he is not content as to the reason for those few things. He would like to start with the idea that the few things are not purely arbitrary postulates but, after all, are very reasonable. He likes to adorn those postulates with some kind of a raiment which makes them seem as though they had come from somewhere, even though he does not know where. When he is presented with Newton's law of gravitation in the cold-blooded form that the acceleration of a particle is to be calculated as the sum of a number of contributions associated with the various elements of matter in the universe, each element producing an effect inversely proportional to the square of its distance from the particle in question, it is not enough for him that in this simple postulate he has said something from which very many beautiful things may be deduced. It is not enough that this postulate gives for him the orbits of the planets, the times of recurrence of eclipses, the times when comets return, and a hundred other things. He must adorn it with physical significance, so he calls the left-hand side the rate of change of momentum and the right-hand side the force, and having called the right-hand side the force he naturally inquires, "Why should there be a force?" "Now," he says, "I will look for the reason for this force in some of the other things that I know about." And he thinks of a piece of elastic, and he sees how that exerts a force, and he wishes he could see that force which the planets exert on each other as arising in the same way as a piece of elastic exerts a force upon a stone which is fastened to one end of it when the stone is swung around in a circle with the other end of the elastic held in the hand. If I ask him why the elastic pulls, he will probably tell me that, while we do not know all about that, we believe that it is due to the fact that the elastic is composed of a lot of little molecules, and when they are separated from each other they tend to come back again. But I proceed to inquire why they tend to come back again when they are separated from each other. He tells me, "Well, although we say *separated*, we do not really mean separated. We believe that between the molecules there is a medium, and when this medium is stretched it tends to return to its original condition." But I inquire why it does this, and he tells me that it is because the medium is endowed with elastic properties. But I ask him what he means by that, and he tells me that it acts like a piece of elastic, and so I ask him why a piece of elastic pulls, and he tells me that it is because it is composed of a lot of little molecules, and so on, *ad infinitum*. You see all that even a successful appeal to the elastic could hope to do would be to show that this thing, gravitation, which we think we do not understand, acts in the same sort of way as that other thing about the elastic, which we also do not understand but think we do. There is a sort of a unification of ignorance in the matter; but this unification of ignorance is not something which should be despised—it is one of the plans and purposes of physics. Thus, we like to picture the atom working as a dynamo works, or as a motor works, or as the ripples on a bowl of water work—something which has become familiar to us in our youth and concerning which we have become accustomed to be satisfied. We met the elastic at an age when we were too young to question its actions, and when we had become old enough seriously to think about it, it had become so familiar to us that there seemed no necessity to think about it any further. The elastic was a little god, the origin of all things, who himself needed no explanation for his origin. And so physics, like everything else, has its starting point, its postulates, and the postulates of physics are its gods. Some appear artificial, as did the plain statement of the Newtonian law, and others mask in a guise of reasonableness, as did the elastic, but a reasonableness which very frequently evaporates in the sunshine of more complete thought.

In order that certain postulates shall be agreeable to a physicist of limited sophistication, it is necessary

that they should be of the same kind as those associated with the behavior of circumstances and things to which the physicist has become accustomed and which he has been willing to accept on the basis of long acquaintance, formulated, probably, at a time before he had decided seriously to think about the matter. Then they must be associated with things which he calls "real." And what does he mean by the "reality" of things? We get a crude notion of his feeling in this matter by asking him to accompany us to a spiritualistic séance and seeing what he is willing to accept as to the reality of the ghost. It is not sufficient that the ghost has properties and produces phenomena. It is not sufficient, even, that the ghost shall produce material phenomena, such as the ringing of tambourines or the feeling of a draft. It is not sufficient that this entity, the ghost, shall merely be defined so as to account for all the phenomena that we are primarily interested in. It is necessary for his reality in the rather ill-defined accepted view of the matter that he shall have certain other properties which are not concerned with the activities which are the main purposes of his function. He must be capable of being seen; he must have weight; he must be felt when he is touched, etc. As a matter of fact, we should be more impressed with a ghost who had these properties and who did not do anything than we should be with one who was without them but was possessed in other respects of all sorts of remarkable powers. In our search for reality in physics, we do very much the same kind of thing. Bodies which are separated from each other appear to have effects upon each other. We discover the laws according to which these effects operate. We should like these laws to be interpretable in terms of a medium which we can think of as real. We shall be able to think of it as real if it has inertia and elasticity in the sense that a solid has inertia and elasticity, even though we have to take these properties as fundamental starting points in the case of the solid, and are unable to trace them to some ultimate source which of itself needs no explanation. At one end of our scale of satisfaction, we have a case where it is possible to account for the properties of the hypothetical medium without any departure whatsoever from the properties of the things that we have heard, smelled and felt, and ranging from this end of the scale to the other extreme end of complete expression in terms of abstract formulation, we have a graded sequence of possibilities which are apt to content us less and less, the further they are removed from the end of complete satisfaction. The other receives a slight blow to its prestige of reality when it is found that though it can operate according to inertia and elasticity, the elasticity is of a different type from anything that we happen to know of in the things that we have touched, seen, etc.

I have often been impressed by what I have sometimes called "the irrelevance of the obvious." I can illustrate the point by considering the case of a problem which I present to a small boy concerning a ball which is hurled vertically into the atmosphere with a certain speed. I ask him how long a period will elapse before it reaches its highest point. The boy comes back to me and tells me that he can not work out the problem, because I have not given him enough information. I ask him what information he would like to have, and he tells me that he would like to know the color of the ball. I tell him the color does not matter, but he probably does not like that, because some of the substantiality of the ball has vanished with its color. He asks for the weight of the ball, and I tell him that that does not matter. And I add that I will even withdraw my statement that it was a nice, round ball and will refuse to state what its shape actually is. Then, if he is materialistically minded, he will blow up entirely and demand how he is to work out the problem at all if I will not tell him anything about it. I finally tell him that the ball is red, that it weighs ten pounds, and that it is really a nice, round ball. He then goes to the blackboard, draws a circle, paints it red in his mind's eye, puts a "10" inside it, and works out the problem. When he brings up the result, I ask him where he utilized the redness of the ball. He looks through the calculations and finds that he did not use it. I ask him where the weight comes in. He looks through his calculations again and finds that he did not use that either, or if he did, it canceled out. And so with the roundness of the ball. Then I admonish him not to ask me a lot of unnecessary questions again. But I think I hear you sympathizing with this poor student. You will say to me, "What harm does it do to tell him that the ball is red, since the redness does not matter? Why did he sin in thinking that it weighed ten pounds, if, after all, its weight is irrelevant?" Well, I agree that in this case, perhaps, no particular harm was done; and yet I have a suspicion that if I allow him to think that the ball is red, some day he will come back to me with a problem which he is unable to solve, because, perchance, the ball in that problem may be blue. Then I shall have the trouble of bringing up past history to explain why it was that the redness of the ball really did not matter in the original problem. But if he has had this vision of redness for a sufficiently long time, it will have become so ingrained in his consciousness that he may be totally unable to think if he is deprived of it.

It would be very difficult to give a definition of what constitutes reality in a general sense which would satisfy everybody. For few who desire reality the greatest could state in words just what it is that they do desire. The best one could probably do

would be to make a list of things and phenomena which he would agree were real. Then if one would take any of these things and gradually remove from it all the appendages which even our disciple of reality himself would admit were unnecessary for its function, it is probable that he would gradually find his conviction of reality vanishing with these appendages, until by the time that he had left all that he himself would claim were necessary, he would have something which, in terms of his own mode of thought, he would have to call "unreal." Even as the sight of the ghost, of his flowing robe, of his obvious weight, are the symbols of reality to the onlooker—though they perform no part of his function—so, frequently, in the physical world, those things which constitute that vague thought of reality are things which play no part in the phenomena which are the main interest of discussion. Sometimes, when systems and phenomena are of such an abstract nature as to shock our material senses, we even go to the extent of providing a curious kind of comfort for ourselves by garbing them in the very clothes of what constitutes reality to us, and then deny to those clothes themselves any of the ordinary properties of clothes, in order that they may not give any trouble. Thus, in the Bohr atom, we are apt to feel that we have something approaching reality in our model of electrons going around a nucleus in planetary orbits. We know that the laws according to which the electrons must operate are different from those of classical electrodynamics; but I think it safe to say that a great many physicists feel much happier in thinking of the Bohr picture than in thinking of the picture of wave mechanics, for example. And yet, what a curious situation we have here. In the Bohr atom, the only model that we have of anything is the model of the part that does not do anything. The Bohr theory gives us a beautiful picture of electrons moving in planetary orbits. The thing is delightful to look at on the blackboard; but, unfortunately, this beautiful model—what is going on on the blackboard—is just the part which is totally unobservable to our senses or in our apparatus. Only when the atom radiates do we get anything observable, but of the radiation mechanism the Bohr theory says nothing. It is true that by talking about the different states of the atom when it does not do anything we can set up a formal procedure for calculating what results follow when it does do something, but there is no picture of the process. In fact, everything which the picture would suggest in the matter of radiation is forbidden to happen. The planetary orbits are not a picture of the process any more than a conglomeration of railway stations presents pictures of the scenery on the journeys between them. A process of calculation we have, it is true; but, for the rest, it is simply a picture thrown in on the side to make us happy.

Let me cite a parable to indicate the condition of mind of one who thinks that in the Bohr atom he has a real, satisfying model. Suppose that I should encounter a strange monstrosity whom I was pleased to call a man, because he looks something like one. But suppose the monstrosity has all sorts of peculiarities. I look at him and exclaim, "See what muscles he has; such a being should be able to swing a five-hundred-pound hammer with ease. See how large his eyes are; such a being should be able to see the most distant stars without a telescope. See the length of his legs; such a man should have a stride which would carry him along at the rate of fifty miles an hour." And suppose that when we came to examine this being, we found that he could not lift a pepper pot, and that what he could lift in comparison with others of his kind depended not upon the size of his muscles, but upon the length of his hair multiplied by the diameter of his eyeballs. Suppose that his vision was no keener than ours, and such as it was, depended not upon his eyes at all but upon the distance between his toes divided by the diameter of one of his eyelashes. Suppose that his speed in walking depended not upon his legs but upon the length of his little finger. It might be that from his arms, legs, eyes, etc., we could make up a way of deducing what he would do under given circumstances, just as we can from the planetary orbits of the Bohr model make calculations about the radiation; but we should delude ourselves if we took comfort in thinking of this monstrosity as a man. And, if we take comfort from the resemblance between the pictures we draw from the Bohr atom and for the planets, we shall assuredly delude ourselves as to the significance of that resemblance also.

One who starts with certain preconceived pictures of how nature works may usually, with sufficient trouble, force those pictures to fit the frame of nature to some extent, but very likely there will be many loose joints and bizarre fits. If one takes two landscape paintings and superposes them by painting one over the other, he will get something which looks unlike any landscape. It is, of course, conceivable that by superposing a sufficiently large number of suitable landscapes, one might get something which looked like a circle, or a straight line, but to hold to the dogma that straight lines and circles are all fundamentally built up out of landscapes is to invite trouble.

If I should define the number of dimensions to be associated with a system as the number of numbers which it is necessary to assign to the system in order that, by writing down differential equations between them in terms of some arbitrary parameter, I could

set up a satisfactory scheme of mathematics with a one-one correspondence to the facts of nature, it would probably be contended by many that I was talking in a very abstract manner and that such a definition of the number of dimensions of a system was highly artificial. The materialist will probably tell me that these dimensions are not real dimensions at all but merely mathematical abstractions. I shall ask him what he means by "real dimensions"—in what sense, for instance, does he regard space as three-dimensional? He will probably illustrate what he means by telling me that he sees me standing here, a three-dimensional being, with length, breadth and thickness, and that in this sense I have very obviously three dimensions. Alas! I shall have to point out to him that the impression which he gets of me is obtained through a two-dimensional image on the retinas of his eyes; that he sees me twice over, once in each eye; that he sees me upside down and that what the left eye sees the right-hand side of his brain interprets. The interpretation of the phenomenon of seeing your president here delivering this address is really a terribly complicated business.

Frequently, the development of a subject—such a subject as electrodynamics, for example, takes place in the first instance through experiments of a large-scaled nature performed upon more or less crude apparatus. As a result of this, we form concepts of a subject founded upon large-scale phenomena. We form concepts of electric and magnetic fields as the forces on magnetic poles and on unit charges. We form the concept of forces on the charges in the sense defined as the product of the masses associated with the bodies on which those charges exist, multiplied by their acceleration, etc. Later, as science develops, it becomes necessary to extend and generalize the mathematical equations which are the basis of the theory. They become generalized to apply to situations where the quantities cease to have meaning in the sense of their original definitions. There is no meaning to the field at a point inside an electron when defined as the force on a unit pole, nor, indeed, is there meaning to the force on the unit pole itself, even if we had it and could put it in the electron, when the force is defined as the mass of the pole multiplied by the acceleration. There arises a whole new formulation of the theory, in which these fields become defined in different ways in relation to a new set of starting points, etc. The things which were before the simple concepts, the masses of the bodies on which the charges were placed, now become quite complicated and elaborate parts of the theory. Those things which seemed to endow the subject with such elements of tangibility when considered on an engineering scale dissolve in meaning, leaving only the fundamental concepts which, as a matter of fact, were the only things which really counted in the engineering problems themselves, where, however, they remained hidden in the glittering robes of a spurious reality. And so, to the critically minded, there appears a reality in the new artificiality and an artificiality in the old reality. The process of generalization is not apt to become one in which the new forms a complication or extension of the old, but rather one in which the old is a rather vague, somewhat incomplete and illogical application of the principles of the new. When, as the result of a merciless stripping of irrelevant adornment from the laws of physics, we arrive at a spectacle unpalatable to the intellectual taste, let us ask ourselves whether this cold remnant does not contain the whole essence of the laws in the sense in which they are actually used. There is no harm in stimulating our intellectual activities by adorning our thoughts with irrelevant appendages, provided we use these appendages as our servants and not as our masters. It is true that the human mind is a mechanism which requires a spark to set it off. The mere assurance that it has all the necessary wherewithal to think with is not of itself sufficient to set the thinking going. If one man finds that a glass of wine is good for his mental activity, let him take it. If another finds that a model accelerates his thoughts, let him use it, however illogical and fantastic it may be, so long as he uses it only as a stimulant to thought and does not impose upon the structure which he has created some of the requirements of the model itself which may be inconsistent with the fundamentals of that structure. It is in the trouble caused by the requirements of these irrelevant characteristics of the model that the danger of an artificially created reality lies. Thus, in pondering upon the physical nature of an ether, for example, while we would strenuously deny that our picture of the all-prevading medium was anything like that of water, many of us will supplement the cold statement of the properties of the medium with vague shadows of substantiality, concerning which only the most dire intellectual torments would bring us to confess, even to ourselves, that we were semi-consciously thinking of the taste of ether, of the smell of ether, of its boiling point, etc.

I have spoken of a theory in mathematical physics as comprising the formulation of a branch of mathematics in which there is a one-one correspondence between the essential elements in the mathematics and the observable phenomena of nature. One of the main differences between the materialistic forms of theory and the more abstract forms is that in the former we seek this one-one correspondence between every stage of the mathematical work and some phenomena of the observable kind, while in the latter form of theory the correspondence only exists be-

tween the mathematics and nature at certain isolated points. I may perhaps illustrate this matter in a very simple way as follows. Suppose that a mathematician wants to write the equation of a parabola in what he calls the parametric form. He writes:

$$x = us, \quad (1)$$

$$y = \tfrac{1}{2}gs^2. \quad (2)$$

We then know that by eliminatings from these two equations, we can obtain the result,

$$y = \frac{g}{2u^2}x^2,$$

which is the equation of a parabola. But, for some purposes, it is convenient to leave the equation in a parametric form as expressed by (1) and (2). Now, if the physicist were presented with equations (1) and (2), he would ask what s was. The mathematician would tell him that s was nothing in particular, but simply an intermediate symbol, introduced for the convenience in relating x and y. But the physicist would be very unhappy about this. He would want a correspondence between this symbol s and something in nature. Happily, in this case, he has it. If he changes s to t, he can think of the whole problem as one of a falling body initially projected horizontally in the horizontal direction x with a velocity u, so that x equals ut, and allowed to fall vertically with an acceleration g, so that $y = \frac{1}{2}gt^2$. It is not always possible in physics to do what was done for s in this case, namely, to find a real correspondence between every symbol in the mathematics and some similar measurable quantity in the physics. At least, it is not possible to find it in a direct way. One can usually express one of the abstract quantities itself in terms of some of the measurable things in such a way as to evaluate it, but this usually does not satisfy the materialist. He likes to have the meaning of the quantity staring him in the face. Most of the quantities which cause trouble in mathematical physics, because of their abstract nature, may really be regarded, in a sense, as parameters in the equations—parameters introduced for the convenience of expressing the relationship between the observable quantities in a more convenient form than by some direct relationship. Considering, for example, the case of electrodynamics, it would be perfectly possible, but extremely cumbersome to express the motion of one electron as a function of the positions and motions of all the other electrons. The whole scheme of relationships assumes a much more elegant and convenient form when intermediate quantities called electric and magnetic fields are introduced as intermediaries. The electric and magnetic fields are really defined by the circuital relations in terms of the motions of the entities themselves; and then, by additional equations, those motions are again expressed in terms of the fields, the whole process being the equivalent of expressing the motion of the electrons in terms of each other.

If you should wish to force me to say what I would deem to constitute reality in a theory or in the conceptions which form a part of a theory, if you should demand of me a statement as to whether the concepts involved in a certain theory were real or artificial, it would, first of all, be necessary for me to ask for a precise definition of reality. I surmise that you will have difficulty in giving such a definition. Or, if you gave one which was satisfactory to yourself, I doubt whether it would be satisfactory to every one else. We should all probably find ourselves in the position of the group of statesmen who were discussing what Mr. Balfour meant when, at the time when tariff reform was a very touchy matter, he had ventured one of his customary non-committal phrases concerned with what he called "broadening the basis of taxation." Every one wanted to know what it meant and how it affected his own particular interests. A cartoon represents Politician A asking B what he understands by Mr. Balfour's statement, "broadening of the basis of taxation." B says, "Well, I mean exactly what Mr. C means." So they go to Mr. C, who says, "Well, I mean just what Mr. D means." Finally, they go to Mr. Balfour, who gives another of his famous statements: "Well, I mean exactly what we all mean." One might make definitions galore. He might define a quantity as real, provided that it obeyed the equation of continuity. He would then be comforted for the moment by thinking of a gas and would say, "Ah! My quantity now behaves just like a gas. If the amount that goes out of the room is greater than the amount that comes in, then the quantity in the room must decrease by an amount equal to the difference. Such stuff is really real. He tells that to the high-school boy who is quite satisfied about it until he is told that momentum also obeys the same relation. He is not so happy about the reality of momentum. We tell him that he will feel that that is also real when he gets older. We, at any rate, glory in a sophistication where momentum satisfies our mathematical criterion of reality and also our undefined sense of the fitness of things. Then the specialist in wave mechanics comes along with the mysterious quantity $\psi\psi^*$, concerning which he also has an equation of continuity. He may wish to present this quantity as a candidate for reality, but the materialist feels that he has been caught and that the claim is not fair. The wave-mechanical person demands to know why the claim is not fair. The materialist fishes around a bit and then thinks that he sees why

the claim was not fair. He says, "You can not measure $\psi\psi^*$, whereas I can measure the density of a gas." But the wave-mechanical person contends that he can measure $\psi\psi^*$. It is a quantity which he can express in terms of the probability of the existence of an electron at a certain place. The materialist claims that this isn't any sort of a quantity at all—nothing like as tangible a quantity as is density. But, after all, if I ask him what is meant by density, he will probably tell me, "it is the mass per unit volume." Then I must ask what the mass is. If he defines mass by a momentum specification in which the measure of the mass is obtained through the velocity that it imparts to a standard of mass when it collides with it with an assigned velocity, then I begin to doubt whether the quantity "mass" is any less mysterious than the one called $\psi\psi^*$. On the other hand, if he defines it in terms of the weight, he has to rely upon a comparison with another body made through the agency of the mysterious force of gravitation, in order that he shall be able to say what he means by the mass. Of course, I am quite well aware of the fact that he has in his mind that high-school-text-book definition of mass as the quantity of matter in a body, but I fear that he would be hard put to it if I demanded an explanation of what he meant by "the quantity of matter," apart from such rather abstract definitions as I have cited. But perhaps our materialist, having been driven into a tight corner in the matter of the mass or in the matter of the density, will return to the conflict on the basis of the velocity. He will say, "for an equation of continuity, you need a velocity, but what on earth can be the velocity of the quantity $\psi\psi^*$?"[2] I tell him that in the wave-mechanical theory this quantity is defined in a certain way in terms of $\psi\psi^*$, and in a way, moreover, calculated to insure that the equation of continuity shall hold, the only element which finally remains as proved by the properties of the $\psi\psi^*$ function itself being the fact that the space integral of it throughout all space is constant with the time. I point out to the materialist that whenever I have a condition of that kind I can always associate with the quantity, in this case $\psi\psi^*$, a velocity u such that

$$\operatorname{div} \psi\psi^* u + \frac{\partial \psi\psi^*}{\partial t} = 0$$

holds throughout all space and does not require an influx through the infinite boundary to keep matters in order. "Ha, ha!" says the materialist. "Now I have caught you. Your equation of continuity was a bluff. You merely painted this $\psi\psi^*$ in the colors of reality by providing it with a meaningless velocity which enabled it to masquerade as something moving about and retaining its identity on the way. The materialist will claim that the velocity of a gas falls in a very different category. He points out that those little particles of gas are moving about in such a way that he can think of actually measuring their velocity with a meter stick. I could quarrel with him on this basis if I chose. However, I have plenty of ammunition in my pocket and will allow him this much rope; but I must ask him whether he really limits his idea of the equation of continuity as applicable only to situations of this kind. If he is old enough, he will probably remember the days when there were no electrons, at least in the minds of the physicists. He will remember the days when people used to think in terms of homogeneous distributions of electricity moving about in space or in conductors. I ask him whether he ever pondered upon the significance of the velocity of a homogeneous distribution of electricity or of anything else for that matter. I think he will have to confess to me that he did, but if he did, what was he really thinking about when he spoke of the velocity? He will probably ask me to picture one of the particles of the electricity and talk about that. However, I join with him in refusing to admit that there are any particles to talk about. Then he will probably ask me to imagine a little piece of cork placed in the homogeneous distribution of electricity, I confess that I find difficulty in this. However, if he forces me to do so or to let him do so, and talks about the velocity of that piece of cork, I shall never allow him to depart from his definition, and he will have to carry the velocities of corks with him for the rest of his life in all his discussions about electrodynamics. I shall never be willing to let him dissolve that cork and lead me to the stage of deceiving myself that I understand what is meant by the velocity of the electricity at the point at which it disappears. Finally, when the materialist has exhausted himself with his corks, etc., I shall ask him whether in the sense in which he thinks of velocity, the velocity really came in at all in his calculations about electrodynamics in those pre-electron days. He will say, "Certainly, it did! Consider, for example, the equation

$$\frac{1}{c}\left(\rho u + \frac{\partial E}{\partial t}\right) = \operatorname{curl} H. \qquad (3)$$

I ask him whether by any conceivable process he could arrive at the velocity used. He may see the equation of continuity staring him in the face and be tempted to use it, but after our discussion of a little while ago, in which he accused me of doing the same thing, he will not have the face to do that. It will probably occur to him that it is necessary, first of all, to come to a conclusion as to whether he knows what he means by E and H. Well, in those days of

[2] For simplicity, we confine our attention to a three-dimensional case.

macroscopic phenomena, I will allow him to use his unit poles and unit charges for the purpose of talking about E and H. Then, I think, he will have to confess to me that in order to get the quantity u, he will have to get, first of all, the current density, which is the quantity that he is going to regard as the product of ρ into the unknown velocity of u, and he will have to get this current density through equation (3) or its equivalent. As a matter of fact, limiting ourselves to the case of steady currents we all know that when we measure a current we are apt to use a tangent galvanometer and measure it by the magnetic field which it produces, which process is the equivalent of our measuring it by this equation (3) for the particular case where the conditions are steady. Then, having obtained this current density, it will be necessary for him to get the charge density. He will then have to get the charge density by a process which is the equivalent of defining that density through the equation $\rho = \text{div } E$. We are confining our attention to the case where only one kind of electricity is involved. If his picture of the phenomenon is one where there are positive and negative densities moving in different directions, then the argument requires elaboration, but what I have said about it so far is sufficient to indicate the principle which I wish to enforce, namely, that in the sense in which even our materialist would be driven to define his density and his velocity, these qualities would depart very greatly from the concepts which he thought he was thinking about when he thought of the density and velocity of a gas. They would, in fact, involve, in their definition, a degree of what he would call "abstractness" which was in every way comparable with the abstractness involved in the definition of similar quantities in the case of wave mechanics, for example. The difference between the two lies not in the subject, but in the materialist himself. When he thought he was thinking about the electricity, he really was thinking about a gas or water. As the result of this mental stimulation, he was encouraged to carry out the analytical processes permitted by his mathematical equations, with the confidence that he knew what he was doing as he went along. This concept inspired him to make many steps and do many things which he ought to have based on purely analytical procedure, but in which he short-circuited much mathematics by using his intuition. In the case where he was dealing, let us say, with a charge distribution of limited extent which, in his language, was in motion, he has no difficulty in making the calculations necessary to find the time that it takes to go from the point A to the point B. He divides the element of distance ds by the velocity u and integrates. However, he should have been much more careful about this procedure. He ought really to have investigated through his electromagnetic equations the story of the gradual disappearance of the density in one place and its appearance in another, or, at any rate, if he did use the concept of a velocity, he ought to have used it in the sense in which we have conceived of it as being defined.[3] But he does no such thing; he uses his intuition and gets a simple solution to the problem. He, as it were, writes a check based on what he thinks is the intuitional balance of his account in his brain, but it is because of the strength and consistency of his mathematical securities that the check becomes honored. In the whole realm of a mathematically consistent structure, we may think of a multitude of cases which represent conclusions of value. These are linked by paths—sometimes tortuous to follow—of logical reasoning. If we take a lot of different mathematical structures, we shall find that many of the "stations of conclusions" look alike and the railroads that run between them have great similarity. In certain of these mathematical structures, the mind has become so familiar with the relationship of the stations that it can jump from one to the other with alacrity. The jump is philosophically hazardous. It is nearly always right, but the mind little realizes the chances that it has to take. When a similar set of stations makes its appearance in some other mathematical structure, and when the mathematical "railroads of logical thought connections" are the same, our materialist can still jump from one to the other with perfect security. He may be more frightened in his jump, so that in fact he has no confidence to jump at all, because he may be under the illusion that when he jumped before he jumped for certain reasons inherent in what he regarded as the physical significance of the things he was talking about, whereas the only thing which really justified his jump was the mathematical railroad which he had discarded. If he can delude himself with a sufficiently large dose of reality to have confidence to jump in the new realm, he will again avoid breaking his neck, but not for the reasons which he thinks are guiding him, but again on account of the guardian angel in the shape of the mathematical substratum which guards his intellectual adventures. Most of us stimulate our brains to action by the vision of an ill-defined reality at the back of our mental processes. So long as we treat it as a means to an end, all is well, but for him who thinks it has fundamental significance apart from the logical scheme of laws which represent the story to be told, there is trouble ahead. For you who seek reality as something characteristic of certain concepts in physics as distinct from others will find that such reality is but a will-

[3] Complicated now to an even greater extent by the inclusion of the $\frac{\partial E}{\partial t}$ term of Maxwell in equation (3).

o'-the-wisp of philosophy. You may think you have it in your hand but will find that you have merely the shadow of something else. You will pursue that something else; you will touch it, and again it will feel real until you find that your consciousness of its touch is no more than the tingle of your own blood as your hands clasp upon it. Reality is the most alluring of all courtesans, for she makes herself what you would have her at the moment; but she is no rock on which to anchor your soul, for her substance is of the stuff of shadows; she has no existence outside your own dreams and is often no more than the reflection of your own thoughts shining upon the face of nature.

THE CHANGING BACTERIA[1]

By Professor C.-E. A. WINSLOW

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I HAVE been asked to sum up the discussion on bacterial variability to which we have listened this morning chiefly as a compliment to my years, because I have lived through more radical changes in our concept of the bacterial cell than have fallen within the lifetime of many of those present. I am here in the capacity of the innocent bystander, as a witness to the assault which the younger generation of bacteriologists are making on the concepts which in the past were held most sacred.

I happen to have been born in the year 1877, the year in which Nägeli published his famous book emphasizing his concept of pleomorphism, which assumed an almost unlimited variability, involving mutual transmutations between bacteria of the most widely separated groups. His extreme position was quickly overthrown by the work of Koch, Cohn and Migula and their followers, and for a period of thirty years bacteriology was ruled by the concept of relatively fixed and simple bacterial types.

We did know all along that certain variations occurred in the physiological reactions of bacteria, but these variations were for the most part closely and directly related to environmental conditions. We knew, for example, that *Bacillus prodigiosus* produces no red pigment at 37°. We knew that the virulence of many organisms could be enhanced by passage through suitable animal hosts. We knew, too, that virulence could be attenuated by exposure to unfavorable environmental conditions, as in the production of anthrax vaccine by Pasteur. Such variations as this, however, variations which were held to be gradual in their development, quantitative in nature and directly dependent upon the maintenance of the environment which called them forth, did not materially disturb our concepts of bacterial stability.

The first real threat to the older ideas was in the work of Neisser (1906) and Massini (1907) upon *B. coli-mutabile* and in the simultaneous work of Twort on the acquisition of new fermentative powers by organisms of the colon-typhoid group. These observations were confirmed but still regarded as representing exceptional phenomena. Twort's work was, however, continued and broadened by Penfold and elaborated by Ledingham in England, while Eisenberg in Germany was bringing forward convincing evidence of new types of bacterial variability. In 1917, came the work of Weil and Felix on Proteus X 19, in 1921, the recognition of rough and smooth forms by Arkwright; and very shortly the universality of the phenomenon of bacterial dissociation was demonstrated beyond any question. No one can read Hadley's magnificent monograph on this subject without being convinced that we are dealing here with a wide-spread and fundamental characteristic of the bacterial cell.

It is an extraordinary evidence of the fact that we see with our minds and not our eyes that this phenomenon should have remained so long undiscovered. For thirty years, bacteriologists had had rough and smooth colonies staring them in the face and had refused to see them because they did not fit into preconceived concepts of what should be there.

It is now at any rate clear that bacteria of very widely separated groups in pure line cultures, sometimes cultivated from a single cell, break up into two or more different strains with characteristics clearly differentiating them from each other. These characteristics may manifest themselves in cell morphology and capsule formation, in colony type, in biochemical characters and in virulence. The variants may breed true for generations, but in a vast majority of instances they tend in whole or in part to revert to the parent type or to change from one type to another, if suitable environmental stimuli are provided. There is no wild or random variation, for certain fundamental characteristics remain constant and the behavior of a rough or a smooth dissociant of a given species may be just as characteristic as that of the parent strain from which it was derived. The process is not one of mutation in the ordinary biological sense, since it is generally reversible and a true mutation is not. Dissociation is also markedly influenced both in

[1] Address delivered before the Society of American Bacteriologists, Baltimore, December 28, 1931.

amount and in direction by environmental factors. Yet the relation is in no sense direct and specific, as is the case with impressed variations. On a single culture plate, with essentially uniform environmental conditions, we find colonies of various dissociation types growing side by side. Furthermore, dissociation differs from the phenomena of impressed variation in the fact that it is relatively discontinuous, giving us 2, 3 or 4 well-defined strains which are usually much more common than are intermediate forms. (In certain instances, as in Marshall and Jared's recent work on Brucella, the shift from S to R forms fails to show such discontinuity). Finally, in widely separated groups of bacteria, dissociation seems to operate on general parallel lines, as evidenced by the fact that we can apply such terms as "rough" and "smooth" to the resulting strains.

Hadley lists the characteristics of the smooth form as follows, those of the rough form being generally the opposite:

R, Mutant Type

Agglutinative growth in broth.
Sedimentary suspension in 0.8 per cent. salt sol.
Often expansive growth on agar.[2]
Colonies rough, irregular, flat.
Seldom generates secondary colonies.
Agar growth harder, translucent.
Agar growth seldom so fluorescent.
Agar growth nonpyocyanogenic.
In motile species, nonmotile.
Noncapsulated.
Biochemically less active.
Often pure R; may have some O or S.
Lacks the "specific soluble substances."
Flocculent precipitate only in immune serum.
Slightly or nonvirulent (nontoxic[3]).
More common in carriers and convalesc.
More common in chronic infections.
More resistant in aging.
Less sensitive to bacteriophage.
Product of adaptations.
Tendency to short rods and cocci.
Not transformed in S immune serum.
Transformed to S in R immune serum.[4]
Susceptible to phagocytosis.

In general the smooth strain is the more virulent one; although with the cocci this relationship is reversed; and in general there is a tendency for the less virulent form to predominate, after cultivation outside the animal body.

Meanwhile, the concept of the simplicity of the bacterial life cycle has been upset during the past fifteen years by evidence of an entirely different kind. It had been known from the earliest days of the science that many bacteria went through a spore stage as a part of their life cycle; but this was the only exception to the general rule of binary fission. In 1916, however, Hort in England demonstrated the formation of gonidial bodies forming a definite reproductive stage in the life of colon-typhoid organisms. In the same year Löhnis and Smith in the United States and Enderlein in Germany described a whole life cycle involving complexities like those characteristic of certain of the higher fungi; and more recently it has been demonstrated that bacteria of widely separated groups may pass through a filterable stage which is later able to reproduce the original type of cell from which it was derived. The phenomena of gonidia formation and of filterability are now established beyond any question; and such work as that of Morton Kahn, in which the entire process of cell-disintegration and reintegration has been observed under the microscope in single-cell cultures, should convince the most skeptical. Evidence for a form of sexual reproduction in certain bacteria is also impressive, if not completely conclusive. Kuhn and Sternberg have recently attempted to explain all the phenomena of bacterial variation as due to the presence and influence of new hypothetical protozoon-like organisms which they call Pettenkofer bodies; but their assumptions seem wholly gratuitous.

Hadley and others believe that the two sorts of phenomena which have been here discussed are fundamentally related and that dissociation is the result of sexual reproduction. I must confess that I am not yet convinced of any such essential relationship and I believe it is wiser at present to recognize two distinct types of bacterial variation, which we may for convenience call dissociative and cyclogenic, and to leave the question of their interrelationship open for further exploration.

I must confess that it is not easy to distinguish clearly and sharply between these two phenomena on philosophical grounds. Both represent essentially discontinuous and reversible changes in morphological and physiological characteristics. Both are influenced by the environment; yet one senses a certain fundamental difference between the two types of variation. The cyclogenic variation, as its name implies, suggests an analogy with the normal life cycle of the higher forms of life, while dissociation similarly recalls mutation in the higher forms. As stated above, however, this latter analogy can not be a true one because of the reversibility of dissociation phenomena and also because discontinuity in dissociation is only relative. For the present we can perhaps only recog-

[2] Apparent exception in case of *B. proteus.*
[3] Relation to toxicity established only for *B. diphtheriae* and *B. enteritidis;* suggested for *B. dysenteriae* Shiga and for *B. botulinus.*
[4] Established for *B. subtilis.*

nize the two distinct types of variation and hope for further light as to their significance and mutual relationship. I do not, however, believe that more than two new types of variability need not at present be postulated; and I would suggest that efforts be made to correlate any fresh phenomenon of variation with either the cyclogenic or the dissociative type of variability, before introducing new terminology or new philosophical concepts.

Bacteriophagy I conceive as a third and quite distinct phenomenon, arising from the influence upon bacteria of a self-perpetuating substance (probably an organism) of independent origin. I can not believe with Hadley that bacteriophage is a result of dissociation; nor can I agree with my colleague, D'Herelle, that dissociation is necessarily and always a result of bacteriophagy. Rough strains are commonly lysogenic and a symbiosis between bacterium and bacteriophage might explain the appearance of certain "rough" characteristics. It seems quite certain, moreover, that action of bacteriophage is one of the commonest and most powerful stimulants to both cyclogenic and dissociative variation. It may well be, however, that other incitants would prove to be of equal generality if they were tested as extensively.

As one who has been long interested in systematic bacteriology I would point out that all this new knowledge does not involve a return to Nägeli's pleomorphism nor does it invalidate the conception of bacterial specificity. The fact that under certain conditions the colony of typhoid bacilli produces papillae of lactose-fermenting organisms is just as definite a characteristic of the typhoid bacillus as the fact that most of its cells fail to ferment that sugar. What we are getting from these studies is not a negation of bacterial specificity, but a conception that the characteristics of each species are vastly more complex than we had supposed.

Nor is there any danger that these new discoveries will overthrow the fundamental bases of epidemiology. It is just as true as it ever was that typhoid fever is normally caused by the presence of the Eberth bacillus and diphtheria by the Klebs-Loeffler bacillus. The simple concepts of specificity have proved their value. They are relatively, if not absolutely, sound, as proven by the empirical results obtained through their application. On the other hand, the new knowledge opens enormous possibilities of explaining obscure phenomena and of obtaining new methods of defense against communicable disease. The serological relation of Proteus X 19 to typhus fever and of *Salmonella suipestifer* to hog cholera, the association of Pfeiffer bacilli and streptococci with influenza and poliomyelitis may all perhaps be elucidated by future discoveries in this field. It may be that exposure to a non-virulent but immunizing phase of the diphtheria bacillus and the streptococcus may account for the development of immunity which takes place during childhood and which always seemed a difficult phenomenon to explain on accepted principles. The use of nonvirulent dissociants like the bacillus of Calmette and Guerin may widen our resources in the production of artificial immunity.

The brilliant work of those who have presented papers to-day, and of their predecessors, has opened up new fields in bacteriology which are full of hope and promise. They have given us a new concept of the marvelous properties of the organisms with which we work. They have multiplied the fascination and the practical possibilities of our science.

SCIENTIFIC EVENTS

THE ANNUAL EXHIBIT OF THE BRITISH PHYSICAL AND OPTICAL SOCIETIES

THE twenty-second annual exhibition held jointly by the Physical Society and the Optical Society was held at the Imperial College of Science and Technology, South Kensington, in January. The exhibit was open to the general public. The London *Times* reports that the bulk of the floor space was occupied by the exhibits, many of them new, of the firms of scientific instrument makers. On the top floor and at a few other stands, however, there may be seen groups of apparatus displayed by some thirty contributors to the Research and Experimental Section.

The exhibitors here include many official bodies, such as the Meteorological Office, the Government Laboratory, and the National Physical Laboratory, the last of which showed a great variety of apparatus, including an interesting optical test of the effects of glare on the eye. The Post Office Engineering Research Station showed, among other apparatus, an "artificial ear," which has acoustic effects upon a telephone receiver equivalent to application to a human ear.

Mr. R. G. Bateson, of the Forest Products Research Laboratory, exhibited a direct-reading humidity meter operating on the wet and dry bulb thermometer principle, and enabling calculations of humidity to be made by inspection without reference to the usual tables or charts. A new humidity control apparatus for use in small testing rooms and laboratories was shown by the Wool Industries Research Association, together with a meter for testing the permeability of fabrics by the air, an extensometer for testing the elasticity of individual fibers, and a combined electrical

heater and temperature measuring sheet which will enable cloth-pressing to be carried on by less empirical methods than at present. The Rothamsted Experimental Station displayed a "pachimeter" made out of meccano parts for testing the plasticity of soils and clays. This, it is thought, may be useful also for millers and bakers in the preparation of flours.

Apparatus for studying speech visually were to be seen in many parts of the exhibition, the most interesting, perhaps, according to the *Times*, being that developed by Professor E. W. Scripture and the adaptations used at the Speech Laboratory of the West End Hospital for Nervous Diseases by Miss F. Janvrin. Speech curves, it appears, can be produced upon smoked paper, so as to provide clearly recognizable tests for the presence of such diseases as epilepsy, disseminated sclerosis, or general paralysis when the symptoms can not be directly recognized in listening to the patient's voice. Another interesting application of physical apparatus to physiological problems was shown by Professor A. V. Hill, in photographic exhibits of the minute heat-production curves which are produced by the stimulation of a nerve or the twitch of a muscle.

In the trade section the experimental sound-film projector for sub-standard (16 millimeter) film shown by the Ensign Company, the "tong-test" for alternating and direct current measurement produced by Crompton Parkinson, and the giant epidiascope with a throw of 70 feet made by W. Edwards and Company were typical of the more popular apparatus.

THE FEDERAL APPROPRIATIONS FOR AGRICULTURAL RESEARCH

SCIENCE SERVICE reports that items in the agricultural appropriation bill, which were reduced below the Bureau of the Budget estimates for 1933 by the House Committee on Appropriations included many lines of scientific research, which had already felt the pruning knife of the agricultural chiefs and the bureau.

The Bureau of the Budget estimates had pared the $235,664,694 agricultural supply act of 1932 down to $186,243,405—a decrease of more than $60,000,000. Much of this was concerned with decreased appropriations for road building and relief loans—in fact almost all of it except about $10,000,000 to be cut from regular departmental activities. Actual savings of more than three millions, to revert to the treasury, were planned in various agricultural bureaus.

The house appropriations committee cuts from budget estimates, amounting to an additional ten million dollars, were made all along the line, and touched the following scientific investigations:

Administration of agricultural experiment stations, $13,646. (Bureau of the Budget had recommended increase.)

General weather service and research in Weather Bureau, $30,500. (Already cut $71,482.)

Aerology, $26,900. (Already cut $225,000.)

Animal husbandry, $20,505. (Already cut $29,495.)

Diseases of animals, $6,000. (Already cut $31,050.)

Dairy investigations, $9,900. (Already cut $69,442.)

Barberry eradication, $3,600. (Already cut $177,140.)

Cereal crops and diseases, $3,700. (Already cut $19,575.)

Citrus canker eradication, $2,100. (Already cut $7,900.)

Cotton production and diseases, $2,600. (Already cut $12,100.)

Rubber, fiber and other tropical plants, $1,500. (Already cut $25,031.)

Protection and administration, national forests, $76,500. (Already cut $119,746.)

Forest products investigations, $2,200. (Already cut $25,460.)

Agricultural chemical investigations, $5,600. (Already cut $5,851.)

Color investigations, $800. (Already cut $2,500.)

Insecticide and fungicide investigations, $1,100. (Already cut $10,000.)

Soil chemical and physical investigations, $200. (Already cut $30.)

Cereal and forage insects, $4,900. (Already cut $42,500.)

Insects affecting man and animals, $4,300. (Already cut $10,200.)

Agricultural engineering investigations, $6,600. (Already cut $58,550.)

Home economics investigations, $900. (Already cut $12,335.)

Pink boll worm control, $5,600. (Already cut $61,000.)

Gypsy and brown-tail moth control, $5,700. (Already cut $65,000.)

European corn borer control, $500,000. (Already cut $155,000.)

Japanese beetle control, $25,000. (Already cut $45,000.)

Among other cuts made by the Bureau of the Budget and not touched by the committee was one of $40,840 from the soil erosion investigation appropriation last year.

THE INTERNATIONAL CONGRESS OF PURE AND APPLIED CHEMISTRY

THE Ninth International Congress of Pure and Applied Chemistry will be held in Madrid from April 3 to 10. The officers of the congress are: J. R. Mourelo, vice-president of the Academy of Sciences and professor emeritus of the School of Arts, *honorary president;* O. Fernandez, professor at the University of Madrid, member of the Academy of Sciences and dean of the Academy of Pharmacy, *president;* and E. Moles, professor at the university and at the

National Institute of Chemistry and Physics and president of the Spanish Society of Chemistry and Physics, *general secretary.*

The city of Madrid is of interest as a scientific and art center and during the week of the congress Spanish cities, including Seville and Granada, are holding festivals and religious ceremonies and the Andalusia fairs that many members will wish to attend will be in progress. It has therefore been decided to confine the congress to three days, leaving three days during which the meetings of the ninth conference of the International Union may be attended and one day devoted wholly to excursions.

The provisional program follows:

Sunday, April 3

6:30 P. M. Reception of delegates and families by organizing committee.

Monday, April 4

10 A. M. Congress opens.

11 A. M. to 1 P. M. General lecture on "The Raman Effect in Connection with Chemical Constitution," followed by discussion. Sir R. V. Raman, K. W. F. Kohlrausch, and J. Cabannes.

4 to 6 P. M. Meeting of groups and sections. Presentation and discussion of reports.

Tuesday, April 5

Morning left free.

6:30 P. M. Reception.

Wednesday, April 6

9:30 A. M. to 1 P. M. General lecture on "The High Polymers in Chemistry," followed by discussion. K. H. Meyer, A. Staudinger, and W. Bragg.

3:30 to 6 P. M. Meeting of groups and sections.

6:30 P. M. Reception.

Thursday, April 7

Reserved for excursions.

Friday, April 8

9:30 A. M. to 1 P. M. General lecture on "The Chemistry of High Temperatures," followed by discussion. C. Matignon, O. Ruff, and A. Day.

4 P. M. First meeting of council of International Union of Chemistry.

4 to 6 P. M. Meeting of groups and sections.

9 P. M. Official dinner for members of congress.

Saturday, April 9

10 A. M. Meeting of council of International Union of Chemistry.

10 A. M. to 12 M. Meeting of sections and drafting of conclusions.

4 P. M. Closing session.

5 P. M. General assembly of International Union of Chemistry.

Sunday, April 10

Reserved for trips.

The organizing committee has issued a bulletin which includes an account of events leading to the invitation to meet in Madrid. This statement is in part as follows:

In April of 1928, at the celebration of the twenty-fifth anniversary of the Spanish Society of Chemistry and Physics, the president of the society, Professor Palacios, interpreting the heartfelt desire of all Spanish chemists, placed before the King, Alfonso, who was presiding at the function, the plea that he would authorize the delegates of Spain in the International Union of Chemistry to petition that the ninth gathering should be held in Madrid, and, that on the occasion of this meeting, the first post-war congress should take place. This would be the ninth of those held up to the present and would undoubtedly prove to be the best possible means of strengthening the friendly relations among the chemists all over the world. The petition met with a most favorable reception.

In response to this proposal, at the conference which was held at The Hague in July of 1928, by the International Union of Chemistry, the delegation from Spain then present drew up the invitation, which was unanimously accepted.

In view of this agreement, the National Council of the Spanish Federation of Chemical Societies, a Spanish Organization Affiliated to the Union and Comprising the Delegations of the Spanish Society of Chemistry and Physics, of the Society of Spanish Mining Engineers, of the National Union of Chemical Experts, of the Academy of Sciences, of the Institute of Oceanography, and of the National Chamber of Chemical Industries, at that time, negotiated and obtained from the public authorities of the country the entire approval which took the form of a solemn resolution of the government, with the inclusion of credits for the preparatory expenses of the congress in the general state budget of 1929.

AWARD TO DR. CHARLES H. HERTY

THE medal of the American Institute of Chemists has been awarded to Dr. Charles H. Herty.

This medal is awarded annually for "noteworthy and outstanding service to the science and profession of chemistry in America," and is given to Dr. Herty in recognition of his efforts over a long period of years on behalf of American chemists and the American chemical industry. Recently, Dr. Herty has aided the economic rehabilitation of the South by his researches on the paper pulp possibilities of the slash pine. This work is being perfected commercially in the new laboratory built for the State of Georgia by the Chemical Foundation.

In making announcement of the award Dr. Frederick E. Breithut, president of the institute, has issued

the following statement in regard to the work of Dr. Herty:

More than any other chemist, Dr. Herty has held to the ideal of making the United States chemically self-sufficient, dependent on no foreign nation for industrial and pharmaceutical necessities.

Dr. Herty first made himself felt as an economic influence when his early researches at the University of Georgia revolutionized the turpentine and naval stores industry. The Herty method of collecting turpentine greatly increased the productiveness of the pines. Other discoveries corrected wasteful methods of turpentine orcharding and conserved the forests.

Rising to national prominence, Dr. Herty served two years as president of the American Chemical Society, in the all-important years 1915–1916. It was he who awakened the chemists of the country to their national responsibility. He cooperated in mobilizing the chemical man-power of the country and in taking a chemical census which presented the national government with a detailed view of the chemical potentialities of the country.

In 1917, as editor of the *Journal of Industrial and Engineering Chemistry*, he made that publication a guiding influence among American chemists. He began the policy of educating the people about chemistry, and helped form the Chemical Warfare Service as a separate branch of the army.

When the war was over, Dr. Herty insisted that the United States remain chemically independent. He threw himself into the fight for a tariff law which would protect the American chemical industries and insure a supply of medicinals, dyes and other essential chemicals. In carrying on this fight he resigned his editorial position and became president of the Synthetic Organic Chemical Manufacturers' Association. Under the tariff act of 1922, the chemical industry in the United States has grown and prospered.

Another act which furthered public welfare was Dr. Herty's effort in behalf of the Ransdell Bill, which established the National Institute of Health and made health research a direct activity of the federal government.

In 1926 he became adviser to the Chemical Foundation and assisted the foundation's activities in aiding the American chemical industry and in educating the public to the importance of chemistry.

All through Dr. Herty's career he has devoted himself to the welfare of chemists and of the country at large. It is particularly appropriate that we should present him the medal at this time, when his latest investigations, on the southern pines, are just reaching completion. If the South turns from cotton and makes its pines the basis of a new prosperity, that prosperity will be largely due to Dr. Herty.

In addition to Secretary Mellon and his brother, awarded last year the medal of the American Institute of Chemists for establishing the Mellon Institute, other past medalists are Mr. George Eastman, honored for his work in making fine organic chemicals available to the chemists of the country, and Mr. and Mrs. Francis P. Garvan, who established The Chemical Foundation.

The presentation of the medal to Dr. Herty will take place at the annual meeting of the institute, to be held in New York in May.

IN HONOR OF HERMAN LEROY FAIRCHILD

A BUST of Herman LeRoy Fairchild, professor emeritus of geology at the University of Rochester, was presented on January 14 to the university by the Rochester Academy of Science. To honor Dr. Fairchild and to celebrate the occasion, the Rochester Academy of Science and the Rochester Chapter of the Society of the Sigma Xi arranged a dinner and joint meeting at which the bust was unveiled by Dr. Fairchild's granddaughter, Miss Jean Kathryn Fairchild. Professor Floyd Fairbanks, president of the Rochester Academy, presided and the principal address was delivered by Professor Heinrich Ries, of Cornell University, on "The Industrial Value of Geology."

Praise bestowed with such feeling, as can be fully appreciated only by those in attendance, revealed the affection and esteem in which Dr. Fairchild is held by his former students, by his colleagues at the university, and by the members of the scientific societies which he helped to found. In presenting the bust, Professor Fairbanks told of Dr. Fairchild's service to the Rochester Academy of Science, in organizing it on its present basis in 1888, as president from 1889-1901, and as chairman of the publications committee. President Rush Rhees, of the University of Rochester, told of the enthusiasm and untiring energy exhibited by Dr. Fairchild toward both his teaching and his scientific studies since he came to Rochester in 1888. This was further attested by Dr. Harold L. Alling, a former student and now head of the department of geology. Dr. John R. Murlin, president of the local chapter of Sigma Xi, told of Dr. Fairchild's interest and influence in securing the charter for this chapter of which he is now honorary president. Before delivering his address, Dr. Ries spoke of the service Dr. Fairchild has rendered the Geological Society of America as one of the founders, as secretary for seventeen years, and later as president. Dr. Fairchild himself gave a short address.

Some of Dr. Fairchild's 236 publications were placed on exhibit next to the bust.—Q. D. S.

SCIENTIFIC NOTES AND NEWS

THE William H. Nichols medal of the New York section of the American Chemical Society for 1932 has been awarded to Professor James Bryant Conant, chairman of the division of chemistry at Harvard University, in recognition of his work in organic chemistry, particularly in the chemistry of chlorophyll. Dr. Conant will be presented with the Chandler Medal of Columbia University on February 5, when he will discuss "Equilibria and Rates of Some Organic Reactions," and the Nichols Medal on March 11, when he will speak on "An Introduction to the Chlorophyll Molecule."

THE honorary degree of doctor of science has been conferred by the University of Chicago on Dr. William Wallace Campbell, president emeritus of the University of California, president of the National Academy of Sciences.

THE Royal Swedish Academy of Science has elected Professor R. W. Wood, of the Johns Hopkins University, a foreign member, in recognition of his researches in the domain of physical optics.

THE William Wood Gerhard Gold Medal of the Pathological Society of Philadelphia was presented to Dr. Alfred N. Richards, professor of pharmacology, University of Pennsylvania School of Medicine, at a meeting on January 14. Dr. Richards addressed the meeting on "Recent Developments in the Experimental Study of the Function of the Kidney."

THE Order "Al Merito" of Ecuador was recently presented to Surgeon General Hugh S. Cumming, director of the Pan American Sanitary Bureau, in recognition of the cooperation of the bureau with the Government of Ecuador in a sanitary campaign throughout the republic and especially in Guayaquil.

AT the New Orleans meeting of the American Society of Naturalists officers were elected as follows: *President,* Dr. R. A. Gortner, professor of agricultural biochemistry, University of Minnesota; *Vice-president,* Dr. H. J. Muller, professor of zoology, University of Texas; *Secretary,* Dr. E. W. Lindstrom, professor of genetics, Iowa State College (3 years); *Treasurer,* Dr. Sewall Wright, professor of zoology, University of Chicago, reelected.

DR. WINTERTON C. CURTIS, professor of zoology, University of Missouri, has been elected president of the American Society of Zoologists.

NEWLY elected officers of the Botanical Society of America are: *President,* Dr. George J. Peirce, professor of botany and plant physiology, Stanford University; *Vice-president,* Dr. Arthur J. Eames, professor of botany, Cornell University; *Secretary,* Dr. Sam F. Trelease, professor of botany, Columbia University; editors of the *American Journal of Botany,* Dr. Lester W. Sharp, professor of botany, Cornell University, and Dr. B. M. Duggar, professor of physiological and applied botany, University of Wisconsin.

THE council of the American Institute of Consulting Engineers has elected the following officers for the ensuing year: *President,* Mr. Charles Page Perin, of New York, to succeed Colonel Frederic A. Molitor; *Vice-president,* Mr. Edwin F. Wendt, of Washington, D. C., and *Secretary,* Mr. Philip W. Henry, of New York. Mr. J. P. Hallihan, of Detroit, and Mr. A. E. Forstall, of New York, are new members of the council.

PROFESSOR JAMES B. POLLOCK, who for approximately thirty-five years has been a member of the department of botany of the University of Michigan, has presented his resignation, to take effect at the end of the present semester.

DR. P. S. BURGESS, professor of agricultural chemistry at the University of Arizona, has been appointed dean of the College of Agriculture and director of the Experiment Station.

DR. HOWARD D. NEWTON, professor of chemistry, has been appointed dean of the Division of Arts and Sciences of Connecticut Agricultural College at Storrs He succeeds the late Professor George H. Lamson.

DR. MARK V. ZIEGLER, of the U. S. Public Health Service, will spend a year in Raleigh, North Carolina, assisting the State Board of Health in the development of the state and county health programs.

INVESTIGATION of cosmic rays on mountain-tops in snow-water lakes and deep tunnels in Colorado will be resumed in June when Dr. R. D. Bennett, of Boston, will join Professor Joyce C. Stearns, of the University of Denver, and his party.

DR. SAUL B. ARENSON, associate professor of chemistry at the University of Cincinnati, will sail early in April for a tour of the Orient and Australasia. He intends to investigate the status of the broadcasting of scientific talks, as well as to visit certain chemical industries in which he is interested.

DR. PAUL WEATHERWAX, of the department of botany of Indiana University, will leave New Orleans on February 17 for a three months' collecting trip in southern Mexico and Central America. A general collection of grasses will be made and special emphasis will be placed on material having any bearing on the question of the ancestry of Indian corn.

ROBERT VAN V. ANDERSON, formerly of the U. S. Geological Survey, is completing a two years' stay in Algeria where he is engaged in a geological study of the western coast ranges, as collaborator of the "Service de la Carte Géologique de l'Algérie." His address is care of the American Consul, Algiers.

THE first Arthur Lecture of the Smithsonian Institution will be delivered on January 27 in the Auditorium of the Natural History Museum, by Dr. Henry Norris Russell, professor of astronomy at Princeton University. His subject is "The Composition of the Sun."

THE Cutter Lecture on Preventive Medicine will be delivered by Dr. Leslie T. Webster, of the Rockefeller Institute for Medical Research, on Tuesday, January 26, at the Harvard Medical School. Dr. Webster will speak on "Experimental Epidemiology."

THE annual invitation lecture of the University of Iowa Chapter of Sigma Xi was given on January 13, by Dr. Edgar Allen, professor of anatomy and dean of the School of Medicine of the University of Missouri. The title was "Eggs of Man and Monkey." Following the lecture a series of demonstrations of research in progress in the department of zoology were available for inspection of the members and guests of the society.

DR. PHILIP B. HADLEY, associate professor of bacteriology at the University of Michigan Medical School, will give the Eighth Ludvig Hektoen Lecture of the Frank Billings Foundation at the City Club, Chicago, on March 25, on "New Trends in Medical Bacteriology and Immunology."

THE New York Post-Graduate Medical School and Hospital will celebrate the fiftieth anniversary of its founding at a dinner of the faculty association on January 30. The speakers will be Dr. Nicholas Murray Butler, president of Columbia University; Dr. Ray Lyman Wilbur, Secretary of the Interior, and Dr. George H. Meeker, dean of the Graduate School of Medicine, University of Pennsylvania.

ON January 18 the laboratory for the physiology of bacteria was dedicated at Cornell University by Dean Cornelius Betten. Dr. Otto Rahn gave an address on "Cell Structure and Cell Functions."

AT Northwestern University in the department of geology and geography during the first semester of the current academic year visiting lecturers and their subjects included: Dr. G. H. Cady, Illinois Geological Survey, on the "Classification of Coal"; Professor A. N. Winchell, University of Wisconsin, on "The Origin of the Graphites in Veins and Pegmatites"; Dr. G. F. Loughlin, U. S. Geological Survey, on "The Ore Deposits of Cripple Creek, Colorado"; Dr. D. G. Thompson, U. S. Geological Survey, on "The Work of the Ground-Water Section of the U. S. Geological Survey"; Professor A. C. Noé, University of Chicago, on "A Geologist in Russia"; Mr. Thomas A. Hendricks, U. S Geological Survey, on "Oklahoma Coal Measures"; Dr. T. S. Lovering, U. S. Geological Survey, on "The Geology of the Moffat Tunnel" and "The Mineral Belt of the Front Range, Colorado."

THE National Research Council has announced a new committee on accessory minerals of crystalline rocks to study the nature of such minerals, their variation in time and space in igneous bodies and metamorphosed masses and their distribution in sediments. Such studies in the past have shown that some igneous masses have rather characteristic accessory minerals which may be useful in some cases in determining the relationships between isolated outcrops, but very little is known regarding the differences in different parts of a single intrusion or in successive intrusions from a single magmatic source. The committee will attempt to correlate work now in progress and stimulate further study along these lines. At present the committee consists of E. S. Larsen, of Harvard University; J. C. Reed, of the U. S. Geological Survey; J. E. Stark, of Northwestern University; A. C. Tester, of Iowa; A. N. Winchell, of Wisconsin (chairman), and J. F. Wright, of the Canadian Geological Survey.

IN view of the disturbed economic and financial conditions in Europe and America the Council of the Institute of Metals has found it necessary to postpone the meeting, which was to have been held in the United States and in Canada next autumn. The meeting had been arranged with the cooperation of the American Institute of Mining and Metallurgical Engineers, and an assurance has been given that the members will be welcome at such later time as may suit their convenience. The Institute of Metals finished the year with a net gain in its membership of 72—compared with a net gain in 1930 of 38—thus making the total membership 2,232, the highest figures on record.

THE eighteenth annual conference of the National Association for the Prevention of Tuberculosis will be held at University College, London, on July 21, 22 and 23. The occasion will be taken to celebrate the fiftieth anniversary of the discovery of the tubercle bacillus by Robert Koch.

PROFESSOR J. W. GREGORY, as reported in *Nature*, planned to leave England on January 4 with an expedition to investigate some little-known parts of the coastal cordillera of southern Peru and their relations to the Andes and to the recent extension of South

America into the Pacific. It is hoped to examine especially the structure of the part of the Peruvian coast that trends from northwest to southeast, and the geology of the desert belt between that coast and the western front of the Andes. A geological section is also to be prepared across the little-known belt between Ica and the Urubamba valley. The earth-movements and volcanic epochs of the Andes appear to have significant correspondence in date with the main stages in the formation of the Great Rift valley that were discovered in its exploration by Professor Gregory thirty-nine years ago. The expedition will descend from the Andean plateau down one of the head streams of the Amazon, and may return to England across Brazil. Miss McKinnon Wood will accompany the expedition especially to collect fossils. Mr. A. V. Coverley Price will join the expedition at Lima. The work will be helped during parts of the journey by Mr. M. A. Tarnawiecki and Señor Don Alberto Calderon.

THE annual announcement and report of the Mount Desert Island Biological Laboratory for 1932 has been distributed recently. The booklet of thirty-seven pages contains not only the announcements for the thirty-fourth season of the Weir Mitchell Station at Salisbury Cove, and the fourth season of the Dorr Station at Bar Harbor, but also eighteen abstracts of research accomplished in 1931. Application blanks for the use of rooms, and copies of the announcement may be secured from William H. Cole, Rutgers University, New Brunswick, N. J.

THE annual report of Dr. E. D. Merrill, director of the New York Botanical Garden, records that during the year the gardens received special gifts of $18,698.47. Among these were $11,000 from the Carnegie Corporation, $1,918 from Henry W. de Forest to support exploration and investigation of the Louisiana iris and $506 from Dr. N. L. Britton for the purchase of botanical material. Added to the general endowment was a $20,000 bequest from Mortimer L. Schiff. The gardens received an unrestricted gift of $5,000 from the estate of Samuel R. Betts. Among the additions of the year was the K. K. Mackenzie herbarium of more than 60,000 specimens.

THE Department of Botany of Indiana State University has acquired the Charles C. Deam herbarium of 5,000 to 6,000 mounted Indiana plants. The collection was built up in thirty years of work throughout the state by Dr. Deam, who for many years was state forester and is now research forester of the State Conservation Department.

THE physiological institute of the University of Berne has received a gift of $3,000 from the Rockefeller Foundation for the purchase of electrical apparatus.

AN international agreement between Sweden and Denmark for the protection of migratory birds was signed recently by the Swedish Minister for Foreign Affairs and the Danish chargé d'affaires *ad interim*, and the agreement probably will be ratified by the respective governments in the near future, according to information cabled to the Department of State by Mr. John Ball Osborne, consul general at Stockholm. Among other provisions, the agreement forbids the hunting or killing of swans, the use of snares, of shotguns larger than 12 gage, and of automatic shotguns for "sea birds," and the hunting of sea birds from motor boats in the sound (between Sweden and Denmark) and other waters from May 31 to October 1.

A CORRESPONDENT of the *Journal* of the American Medical Association writes that "An accident that calls to mind the catastrophe caused, three years ago, in Belgium by the death-dealing fogs has just occurred in Lyons, although the consequences were less grave. Within a few minutes, a section of the city in which there were many factories and in which many workmen resided found that its atmosphere had become irrespirable. The people suffered from attacks of choking and coughing. The police interrupted all street traffic and ordered all the doors and windows closed. Several cases of bronchitis with choking developed in children, but no deaths occurred. An inquiry soon showed that emanations came from a factory on the banks of the river, in which various chemical products were manufactured and which produced stannic chloride, employed in the manufacture of silk goods, silk weaving being the principal industry of Lyons. This salt is obtained by the direct action of chlorine on tin, the operation being carried on in a closed vessel, because, in the open air, vapors of hydrochloric acid are produced. During the absence of the workmen, a leak developed in a lead joint, and the gas as it escaped attached itself to the droplets of water composing the fog that enveloped the city. The inquiry instituted by the prefect of the department of Rhone resulted in an order prohibiting the manufacture of this product within the city limits, which made it necessary for the factory to seek a new site outside the city. At the time of the accidents that occurred in the Meuse Valley, which cost the lives of sixty persons, the noxious gas was sulphurous anhydride, emitted by the numerous factories in this valley, which is enclosed on all sides and very winding, poorly swept by the wind and subject to frequent fogs. The Belgian inquiry resulted in the suggestion that work in the factories be interrupted on foggy days. One village (Engis), in which the general mortality has been constantly increasing during the past ten years, is threatened with the necessity of closing all its factories."

DISCUSSION

RELATIVE TO THE EXPRESSION "LINE CONTOUR"

ONE reads frequently in the current literature of spectroscopy of "line contours," the intended meaning of the term being, apparently, the intensity graph taken across a spectral line, or, in other words, a small part of the spectral energy curve in the immediate vicinity of the "line" or radiation in question. Inasmuch as technical usage, especially in engineering practice, has endowed the term "contour" with a special connotation of sustained equality, as, for example, in its specific application to lines of equal elevation on a topographic map, the use of the term to designate precisely *what this is not* would appear to be unfortunate.

The word has of course, in general speech, a wide meaning which is not subject to the limitations imposed by technical usage. Thus the Century Dictionary quotes Dr. Oliver Wendell Holmes in illustration: "All her contours and all her movements betrayed a fine muscular development." There is here no implication that horizontal curves only are to be considered, in fact the imagination is left free to engage itself with deflections in all directions, and it would appear permissible to speak metaphorically of the contour of a spectral line in much the same way that we refer directly to that of a body or of a feature of the landscape; but such greater latitude in the meaning of the word when employed in general speech can hardly be taken to justify its technical, and presumably exact, restriction to uses in two conflicting senses. It would seem necessary for the spectroscopist to respect the unquestionable priority of the engineer.

If we were to take over, for the description of a spectral line, the term "contour" in its engineering sense, the "contours" would be points, since the energy curve has but one horizontal dimension. The term "profile" might be appropriated with perfect consistency, since it relates to a vertical section, whose purpose is to show differences in level, and the word has in fact been drafted by Italian spectroscopists (*e.g.*, "Sul profilo del tripletto del magnesio," etc. Publ. della R. Univ. degli studi di Firenze, 48; 28, 1931). A number of other expressions are available, but the actual employment of the term "profile" in one and perhaps more of the principal languages would suggest the propriety of its use in English.

One may perhaps be permitted the reflection that a generation ago the word "contour" would probably not have been adopted by physicists or astronomers to signify something that goes up and down. The apparent readiness with which it is being accepted is perhaps illustrative of the drift of physical science from the influence of engineering, obvious to most of us, and recently remarked by Sir James Jeans ("The Mysterious Universe," p. 19). Is it too late for spectroscopists to select an expression for a simple intensity graph that would conform, or at least not be in conflict, with simple and well-established usage?

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"STRATH" AS A GEOMORPHIC TERM

IN a recent number of SCIENCE, (August 14, 1931, pp. 172–173) F. Bascom proposes the word "berm" as a collective term. It is to include all terraces in stream valleys and on lake and sea shores which owe their origin to relative uplift and dissection of planation surfaces; lateral planation by streams on the one hand and planation by waves on the other.

The purpose of this note is to call attention to an anomalous circumstance. Applying the word "berm" to stream valleys is giving a name to the erosion remnant of a geomorphic feature for which in its original, uneroded condition no technical term exists. The flat bottom of a stream valley produced by degradation has no technical name to distinguish it from one produced by aggradation.

For the lowland which commonly adjoins the low-water channel (or "bed") of a stream we have the word "flood plain." But, as used, this word includes both lowlands formed by cutting away and by filling up ("degradational" and "aggradational" surfaces). The two might be distinguished as "stream-cut"[1] and "stream-filled" flood plains.[2] The latter expression is identical with "alluvial plain" and is therefore not needed. The former, on the other hand, would be inadequate. "Stream-cut flood plains" grade imperceptibly into flat valley bottoms of greater width, often spoken of as "incipient peneplains." These can not have been formed solely by lateral stream-cutting. They may owe their origin to a weathering back of the valley walls after the fashion of other erosion scarps. In fact, the writer knows of no reason why the same process that causes an erosion escarpment to retreat, with no stream running along its foot, should not also produce a recession of valley walls. Whether this inference be justified or not, the fact

[1] Disregarding the veneer of gravel and mud which inevitably covers a stream-cut flood plain.

[2] "Aggraded valley-plain" of C. A. Cotton, who defines "valley-plain" as "a continuous flood-plain." ("Geomorphology of New Zealand," Wellington, N. Z., 1926. Pp. 113 and 197).

remains that, in general, it is not feasible to draw a line between "stream-cut" flood plains and incipient peneplains. Precise description clearly demands a technical term for degradational valley flats.

The writer proposes to restrict the use of the old Scottish word "strath" to this type of flat valley bottom. According to Geikie,[3] popular usage in Scotland applies the word "strath" to "broad expanses of low ground between bounding hills, usually traversed by one main stream and its tributaries." Little violence is done to this original use when the meaning of the word is restricted in a geomorphic sense to those cases in which the flat valley bottom is the result of degradation, first by lateral stream cutting and later by whatever additional processes of degradation may be involved.

At the initial stages of lateral stream cutting, the "strath" is *incomplete,* that is, present only here and there along the valley bottom. Progressive widening renders it *complete,* a broad band of lowland between more or less parallel valley sides.[4]

The justification of this technical restriction of the word "strath" lies in the way it lends itself to the building up of a consistent nomenclature as follows.

In the course of its natural development, a *V-valley*[5] changes into a *strath-valley,* at first with an *incomplete,* then with a *complete strath.*

Rejuvenation leaves remnants of such flat valley bottoms as "terraces." Corresponding to the two types of flat valley floors we have, then, *strath terraces* and *fill terraces,* the latter comprising *alluvial terraces, glacial terraces,* etc.

To the two main types must be added a third,[6] for which the old term *rock terraces* serves well in its original meaning, which refers to ledges on valley sides brought into relief by the superior resistance to weathering of individual rock units.

Stone,[7] Stout[8] and others have used the word "strath" for what the writer proposes to call "strath-terraces." This use of the term has not been accepted widely. Since the writers who have used the term have defined the meaning in which they use it in their publications, there would be no danger of confusion if students of geomorphology should choose to adopt the writer's definition of the term here proposed.

At the mouths of river valleys strath terraces merge into wave-cut terraces. The two are by no means identical and require each a separate, specific name. Collectively they might be called "berms." Unfortunately, this word can not be used in a purely figurative sense since it is practically unknown to most readers. It would necessarily assume the character of a technical term. It is briefer to say "berms grade into partial peneplains" than to say "strath terraces and wave-cut terraces merge into partial peneplains." But it is not any more precise and therefore not necessary to do so. Every addition of a technical term which requires specialized knowledge of nomenclature interferes with the ready understanding of scientific writings by the less specialized reader. If it does not add to precision one might do better to omit a technical term, even though it be as aptly chosen as the term "berm" which, in civil engineering parlance, signifies "a horizontal ledge part way up a slope."

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[3] A. Geikie, "The Scenery of Scotland," 3rd Edition, London, 1901, p. 175.

[4] For the latter case Rich has suggested the term "open valley." J. L. Rich, "Certain Types of Stream Valleys and Their Meaning," *Jour. Geol.,* 22: 469–470, 1914. Dr. Rich agrees to this definition of his term, withdrawing the qualification given in the last paragraph of p. 470.

[5] In technical language we speak of I-beams and T-beams, not of I-shaped and T-shaped beams. It is convenient (not necessary, of course) to speak of V-valleys.

[6] The writer has used these terms for a number of years with his students in the field and classroom and has found their symmetry very useful.

[7] R. W. Stone, "Review of General Geology of Southwestern Pennsylvania in Light of Recent Work," Top. and Geol. Surv. Pennsylvania, 1906–08: 121 ("These benches represent the periods when uplift ceased and the streams had opportunity to widen their valley floors, and the bluffs represent periods of elevation. These later benches are known as 'straths' ". . .)

[8] W. Stout and R. E. Lamborn, "Geology of Columbiana County," Bull. Geol. Sur. Ohio, 28: 41, 1924. ("Strath, a remnant of an old valley," in footnote.) W. Stout, "Geology of Vinton County" *ibid.,* 31: 38, 1927.

OESTRUS

BY his masterly letter on the history of the word "œstrum," Dr. Tyson has justly rebuked me for my somewhat frivolous letter published on March 27.

The point is one of more than passing interest, for it involves the question of the exactness of scientific terminology. A word in general use varies from time to time both in its form and in its meaning. But a scientific term expressly adopted to convey a definite conception should, so long as it does not infringe the laws of priority and of good taste, remain inviolate. Naturally, if the conception itself becomes superseded, the term itself must pass out of use and may be adopted, preferably with some suitable alteration, with a new definition. A recent example of such a change is to be found in the alteration of the older term "lipoid" to the newer "lipid" and its variations, all of which are defined as accurately as our present knowledge of biochemistry allows.

With regard to the word "œstrum," if Dr. Tyson refers to the paper (*Quart. J. Micros. Sci.,* 1900–1,

Vol. 44; see pp. 6–7) in which Heape introduced the terminology under discussion, he will find that the specific point has already been settled. Heape wished to introduce well-defined terms which would differentiate the phases of the reproductive cycle in mammals. Before giving his own choice, he discusses several alternatives, amongst them being the neuter word "œstrum." He rejects this because it already has a meaning in general use which is considerably wider than that which he desires to convey. He therefore takes the Latin word "oestrus" and gives it a clear definition, thereby introducing it as a scientific term. It seems clear, therefore, that there is very good reason for using the scientific term introduced by Heape and not the variant from general usage with a more indefinite meaning.

I feel that there is another reason why we should attempt to respect Heape's terminology, for it is said that to define a problem clearly is half the battle. Heape certainly did this for us and has thus paved the way for recent work on sex hormones which is producing such excellent results, especially in this country.

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BOUGHS, BUTTS AND BEAVER DAMS

IN SCIENCE for December 18, 1931, Mr. Charles Macnamara writes of certain beaver dams he had observed in which "at least 90 per cent." of the boughs had been placed with butt ends *downstream,* instead of upstream, as generally stated in published accounts of the beaver.

There is no question as to the accuracy of Macnamara's observations. On the contrary, as one who long has been an interested observer of the beaver and its works I am quite in agreement with him and feel that many who have written about the beaver, myself included, have, perhaps, when making generalized statements, rather unduly emphasized the position of the boughs in beaver dams. That the butt-end-upstream feature should have been singled out for special mention is probably of psychological interest only, but aside from the question of their accuracy as generalizations such statements tend to create the erroneous impression that this particular position of the boughs is a matter of design on the part of the beaver. Whatever the position of the boughs in a dam the explanation can usually be found, I think, in the conditions confronting the beaver where it is working; and some, at least, of the determining factors are fairly obvious, as, for example, the character of the stream when the dam is first started; the site of the dam and its location with reference to where the beaver gets its boughs; the route taken by the animal (sometimes forced upon it by the terrain) in transporting the boughs to the dam, and other factors.

To illustrate: The position of the boughs is, I believe, most likely to attract attention in dams that have recently been started, especially in such as contain many boughs that are fresh-cut and untrimmed and therefore particularly conspicuous. It is a matter of common observation in beaver country that many dams are started on small or shallow streams that at the time do not permit of transportation of dam materials by water; such transportation becomes possible only as construction proceeds and sufficient water becomes impounded. If, now, the beaver secures most of its boughs at points above the dam site and drags them by way of the creek bed to the dam, most of these boughs will be found on the dam with their butt ends downstream; but if most of the boughs are brought from points below the dam, then they will be found with butt ends upstream. However, many boughs may also be secured off to one side or the other of the dam and, depending in part upon the direction from which the immediate approach to the dam is made, may be found lying crosswise, diagonally or in a variety of other positions.

As the pond enlarges the beaver will very probably bring in most of the boughs from points above the dam, transporting them now by the favored water route, and hence from this time on most of the boughs will doubtless be found, when in place, with butt ends directed downstream.

If any general statement is to be made in regard to the position of the boughs in beaver dams, rather than making any reference to the direction in which the butts are pointing it might be safer merely to say that most of the boughs lie parallel with the stream or the direction of the flow.

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TWISTED TREES

A BRIEF note, under the above caption, by Dr. A. R. Cahn, of the University of Illinois, appeared in SCIENCE several months ago (May 23). In it Dr. Cahn recorded his observations in Ontario, Canada, of evergreen trees having twisted bark and wood. Noting that the twist, in the trees observed, was predominantly to the right, he wondered why this was so. In a letter to Dr. Cahn on June 1 the following explanation was offered.

When a tree is exposed to the wind year after year, it assumes an oval shape, when considered in horizontal cross-section through the leafy portion of the tree. The trunk would be off-center on the side toward the direction of the wind. If the prevailing wind shifts from left to right consistently in the area in which the tree is located, it would seem that there would be

a natural tendency to twist the tree from the left to the right. In other words, it would appear, if this explanation is correct, that the tree is a recording weather vane.

On return to his office this fall, Dr. Cahn replied to my letter and stated that he had come to about the same conclusion and suggested that this note be put in SCIENCE.

NEALE F. HOWARD

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"RICKETTSIA" AND "SYMBIONTS"

GLASER'S comment on our paper[1] (SCIENCE, September 4, 1931, p. 243) does not help to clarify the problem. We are in entire agreement that "Tinctorial properties *alone* (italics ours) do not constitute valid taxonomic characters." We attempted to point out that there are in the insects studied by us organisms which differ morphologically, tinctorially and culturally from other known bacteria as well as from the so-called "Symbionts." Symbionts in various insects probably consist of a variety of bacteria, while the organisms grown by us were uniform in character and appeared in every way to correspond to the description of classic Rickettsia. Therefore, we urged that for sake of clarity we cease confusing "Rickettsia" with "Symbionts." Even conservative bacteriologists, and we count ourselves in that class, have recognized the wisdom and conservatism of dividing the erstwhile genus *Bacillus* into a number of distinct genera. Differentiation of unrelated groups can hardly be considered "splitting," even by the ultra-conservative.

I. J. KLIGLER
M. ASHNER

JERUSALEM,
OCTOBER 20, 1931.

REPORTS

FORMAL OPENING OF THE NEW BERMUDA BIOLOGICAL STATION FOR RESEARCH

THE formal opening of the Bermuda Biological Station for Research, Inc., and the induction into office of its new director, Dr. J. F. G. Wheeler, took place at the new home of the station, "Shore Hills," St. George's West, Bermuda, on Wednesday afternoon, January 6, in the presence of His Excellency, the Governor of Bermuda, General Sir Thomas Astley-Cubitt, K.C.B., etc., and members of his family, members of the Colonial Government and more than five hundred invited guests. Six trustees of the station went down from New York with five guests, two of the Bermuda trustees were present and took a leading part in all the arrangements.

The band of the Northumberland Fusiliers was present in brilliant uniforms, and their music added materially to the pleasure of those attending the function. Special trains on the newly opened railroad conveyed guests from Hamilton and St. George, and Bermuda papers reported the gathering as "the most distinguished and representative seen on any public occasion for many years."

The governor and his party were met by the trustees at the south entrance and were seated in the open court, while the other guests came in by the north entrance and were seated in the verandas and balconies surrounding the court, where the exercises took place. Mr. F. G. Gosling, trustee of the station and for thirty years active in the development of scientific work in Bermuda, opened the exercises with a brief account of the steps which had led up to the present consummation. Thirty years ago, he said, the Bermuda Natural History Society was formed with the prime object of establishing the Bermuda Biological Station and the Public Aquarium. On every occasion the legislature had granted money when approached by the society and this culminated last year when £5,500 was given to assist in acquiring the present site. The Rockefeller Foundation had most generously supported the scheme and had made possible the present splendid development. The result was that to-day Bermula could take great pride in the part played by the government and people in the establishment of the station, which was a purely scientific institution, without any commercial object or aim. He especially commended the action of the trustees in appointing a British subject as the first director of the reorganized station, which statement was loudly applauded. He then introduced Dr. E. L. Mark, of Harvard University, the former director of the old biological station, saying that Bermuda owed him a deep debt of gratitude for his efforts and insistence in helping to bring the station to its present stage.

Dr. Mark, who was warmly greeted on rising to speak, was obviously overjoyed that his dream of so many years had come true, and we give his summary of the history of the station.

Your Excellency, Ladies and Gentlemen: My first impulse is to call you—as many of you long have been—my dear friends.

It is a great pleasure to meet you collectively and to express my keen appreciation of what you have done, both officially and unofficially, to further the interests of the Bermuda Biological Station for Research.

[1] *J. Bacteriology*, 22: 103–114, 1931.

The establishment of the station in 1903 was made possible by the cooperation of several interests. My own participation is due to suggestions made by Charles Eliot, president of Harvard University (who spent the winter of 1900–01 in Bermuda) and to my meeting by chance the late Professor Charles L. Bristol, of New York University, whose pioneer work in advocating the aquarium and station is well known to all Bermudians.

At the invitation of the Bermuda Natural History Society, Professor Bristol and I visited the islands in April, 1903, with a view to aiding in the selection of a site for the station and a public aquarium, the latter to be owned and controlled by the Colonial Government.

After a careful survey of various regions—having in mind the facilities for research, as well as the all-important matter of income from entrance fees to the aquarium—the Flatts Inlet was selected.

By an agreement between New York and Harvard Universities, it was arranged that the station should be established with the aid of funds raised for the purpose by the representatives of the two universities, *viz.:* by myself as director, and by Dr. Bristol as associate director. The laboratory equipment was purchased with funds solicited from graduates of Harvard University, and the station was opened in June, 1903, at the Hotel Frascati. All properly qualified persons (women as well as men), from whatever institution they came, were admitted without fees on equal terms.

For two more summer seasons (1904 and 1905) the work was carried on at the Frascati, and meantime plans for a more permanent location were perfected. Through the agency of the Natural History Society, it was proposed that the Colony should buy a suitable piece of land from the Allen estate near the bridge and should erect on it a building, the lower story of which should be devoted to the aquarium and the upper story placed at the disposal of the biological station. The land was bought, but, unfortunately for the rest of the plan, the War Department of Great Britain about this time suffered a 50 per cent. reduction, which had the effect of greatly diminishing the income of the Colony, so that it was impossible to complete the whole plan. As a result, no work was carried on during the summer of 1906. However, by a fortunate turn of events, Agar's Island became available for this purpose. The society was able to rent the island from the War Department and proceeded (as you know) to convert its "Magazine" into an aquarium. At the same time—the agreement between New York and Harvard Universities having been dissolved—I was invited to resume, as director, the work begun at the Flatts. Accordingly, the laboratory equipment of the station was transferred from the Flatts to a large building on Agar's Island, and those in attendance on the summer session of 1907 were lodged in the cottages on the property.

From the time of the resumption of work in 1907, it had been the aim to provide for investigators at all seasons of the year, but it was not until 1915 that such a plan was fully realized by the appointment of Dr. William J. Crozier as "resident naturalist."

Until April, 1917 (when the United States entered the "World War"), we had remained in undisturbed possession of the island, but unexpectedly we were then notified that we must at once vacate the place, and the whole equipment was transferred to Denslow's Island, where by the generous terms of the owners we were able to reestablish the laboratory.

Some months after the signing of the armistice, *viz.*, in May, 1919, we were allowed to resume possession of Agar's Island. From that time on (the aquarium having been closed), the money so generously granted by the legislature was turned over to the director of the station, who as the representative of Harvard University rented the island from the War Department, and continued so to do till it was sold to Furness, Withy and Company. Since then, through the liberality of the owners, we were permitted to occupy the island till the middle of last June, when the equipment was turned over to the Bermuda Biological Station for Research, Inc. (in accordance with an understanding with Harvard University) and transferred to Shore Hills.

Such is, in brief, the outline of the history of the unincorporated station. As to the scientific results of activities at the station, I must refer you to the nearly 170 articles published by workers at the station. Reprints of these papers are collected into six completed volumes, and one nearly completed, of the Contributions from the station, a set of which you will find in the library on the second floor of this building.

I greatly regret that the time available does not permit me to enumerate the persons who have been most active in supporting financially, and otherwise, the activities of the station, and also the long list of distinguished biologists whose work is evidence of the value of the opportunities provided for them.

In closing, I can do no better than bespeak for my successor, Dr. Wheeler, and the enlarged oceanographic aims of the station a continuance of that loyal and hearty support you of Bermuda have given in the past.

At the conclusion of Dr. Mark's address, Mr. F. G. Gosling requested His Excellency to declare the station open, which he did in the following words:

I do not have to open a dictionary to find out what "biology" means, but I can see at a glance that Bermuda is fortunate to have such an institution as this, and I can think of no more beautiful and convenient site. I have not had the pleasure of visiting the station before, but in common with many others, it will give me great delight to visit it on many occasions in the future. I am not quite sure whether Dr. Beebe will be transferred to this station or whether he is a different biological specimen, but it is most gratifying to know that Bermuda attracts such eminent scientists as now visit and work in this island. I feel sure, too, that to Mr. F. G. Gosling we owe many thanks for his long and enthusiastic work on behalf of this station.

St. George's is a most appropriate site for the institution, [he continued amid laughter] for it was here that the first biological specimens arrived on the island.

It gives me much pleasure to declare the station open, and I wish it every success.

Dr. E. G. Conklin, of Princeton University, president of the Bermuda Biological Station, then spoke of the organization and aims of the Station as follows:

Your Excellency and Honored Guests:

The question has been raised by several of our guests as to how the new Bermuda Biological Station for Research differs from the old one and from Dr. Beebe's Bermuda Oceanographic Expedition. These differences may be characterized in three paragraphs.

(1) In the first place this new station differs from any and all others in this hemisphere or in the entire world, so far as I know, in being really international in its organization. At the initial meeting of the promoters of this new station it was decided to ask leading scientists and contributors to the cause of science in all countries to join in forming a corporation which would be the ultimate body owning and controlling this station. There are now more than two hundred members of this corporation, representing ten different countries. This large corporation then elected a board of trustees to administer the affairs of the station, and in 1926 this organization was incorporated under the laws of the state of New York. There are now twenty-three trustees in our board, representing Bermuda, Canada, England, Scotland and the United States; our organization is truly international and in this respect differs from all other biological stations in this hemisphere.

(2) In the second place the Bermuda Biological Station, like the Marine Biological Laboratory at Woods Hole, is a cooperative institution in the widest sense. While it is the purpose of the trustees to have, as soon as possible, a resident scientific staff, in addition to our resident director, carrying on a program of research in the physics, chemistry and biology of the ocean in the vicinity of Bermuda, this station will also afford opportunities to competent scientists from all countries to pursue independent researches on these subjects, or to join in the general work of the scientific staff. For many years to come, perhaps indefinitely, the major work of the station will consist of such independent investigations of visiting scientists. Already inquiries or applications for research space have come from leading scientists of Canada, England, Scotland and the United States, and there is abundant evidence that the Bermuda Station may count upon the scientific cooperation of scientists throughout the world.

Cooperation is also manifest in the financial as well as in the scientific support of the station. The Bermuda Government by its grant of £5,500 on condition that £50,000 be secured elsewhere took the initial step in this cooperation. The Rockefeller Foundation by its grant of £50,000 met this condition. In addition the Bermuda Government has for several years given £200 a year toward the support of the station and has granted duty-free importation for all equipment and supplies. The Royal Society of London at the initiation of this new organization made a small grant, to indicate its interest in and cooperation with the Bermuda station. Last July the Empire Marketing Board of Great Britain made a conditional grant to the station of £2,500, but owing to the financial crisis has been compelled to postpone this indefinitely. To meet the requirements of our budget in these times of financial stress the Rockefeller Foundation has recently made an emergency grant of $6,000 a year for a period of two years. In return for the use of the facilities of this station the Woods Hole Oceanographic Institution has contributed $2,000 for the current year, and seven universities and scientific institutions in the United States and Canada have agreed to support research tables or rooms at the station. Undoubtedly this number of cooperating institutions can be greatly increased in the near future. In addition to this governmental and institutional support the station has received aid from many individuals, in the form of money, instruments, books and journals, and last, but not least, generous contributions of labor, time and advice. To all these cooperating organizations and individuals we extend hearty thanks and bespeak the continuance and extension of their interest and support.

In order to correct any misunderstanding that may exist as to the purchase of the Shore Hills property and its remodeling for the uses of the station, I may say that the trustees paid to the former owners $80,000 from the Rockefeller grant and that the entire sum given by the Bermuda Government has been spent here in Bermuda for the necessary alterations and repairs.

(3) A third characteristic of the new Bermuda Biological Station is the breadth of the scientific work which is contemplated. This will make it much more than a biological station, for in addition to the usual work of a marine biological laboratory and the extraordinary opportunities offered here for the study of the physiology and life-histories of deep sea animals, we propose to investigate the physics and chemistry of the waters as well as their biology. There is offered here an almost unrivalled opportunity to make an intensive and long-continued study of the drift, temperatures, salinity, oxygen content, etc., of ocean waters down to a depth of more than 2,000 fathoms, and the relation of all these environmental factors to the kinds and abundance of living forms. We expect to study the causes and rate of reef-formation, erosion and sedimentation, but we do not imagine that we can ever raise the level of these islands some thirty feet and thus increase their area approximately twenty times, to their original dimensions, as was humorously proposed to us by the late governor, General Bols. We do hope to make extensive surveys of the ocean bottom with accurate determinations of the force of gravity, bottom deposits, etc., and thereby add to our knowledge of the causes at work in the formation of oceanic islands, but we do not anticipate that such studies will have any immediately practical value. They may never increase the food supply nor the means of living, but they should furnish much food for thought and should increase and ennoble the aims and joys of living.

One hundred years ago a wise and generous English-

man, James Smithson, bequeathed to the United States of America a fortune of more than half a million dollars "to found at Washington under the name of the Smithsonian Institution an establishment for the increase and diffusion of knowledge among men." Thus was created one of the first and greatest scientific institutions of the New World. We who are concerned with the foundation and development of the Bermuda Station wish to emulate the example of James Smithson and to establish here in this British Colony, this island paradise, an institution "for the increase and diffusion of knowledge among men," and to this end we invite the cooperation of governments, foundations, universities and individuals. What other objective is more worthy of high endeavor and great endowment?

This institution is not finished, but only begun. Its long period of incubation under the supervision of Dr. Mark has finally led to its birth in this beautiful and strategic site. Before it, let us hope, lies immortal youth and growth, and ever widening usefulness in the highest service to mankind!"

President Conklin then inducted the new director into office in the following words:

John Francis George Wheeler, B.Sc. and D.Sc. of the University of Bristol and formerly lecturer in the same, student at the Plymouth Laboratory of the Marine Biological Association of the United Kingdom, investigator for the Ministry of Agriculture and Fisheries, zoologist with the *Discovery II* Expedition to South Georgia and South Africa in 1924–27, and again in 1929–30, for the last year in charge of the *Discovery II* office in London, author and co-author of notable monographs on Southern Blue and Fin Whales—By the authority and in the name of the trustees of the Bermuda Biological Station for Research I induct you into the office of director. I charge you to remember that this is a cooperative and not a one-man institution. I bespeak for you and for Mrs. Wheeler the cordial friendship of the people of Bermuda, and I pledge you the support of the corporation and trustees in the further development of this station.

Dr. Wheeler responded in a brief address, in which he said that he was deeply conscious of the great honor and responsibility that had been conferred upon him. He felt that he and his wife had joined a family community in Bermuda and he realized that he was following in the steps of a long line of scientists who had labored to establish the Bermuda Station. There was, he said, a wonderful opportunity for scientific work here, for the ocean abounded with life and it was the duty of scientists to study that life and to make their studies of service to mankind. He spoke particularly of the many correlations of plants and animals in the sea and illustrated this by his studies on the whales of the Antarctic, and then pointed out the bearing of all these correlations on the whaling industry and the manufacture of soap. He ended by expressing his confidence in the support of the board of trustees and of the government and people of Bermuda.

At the conclusion of this ceremony guests were invited to inspect the various aquaria, laboratories, library and living rooms at the station and to partake of tea and refreshments in the main hall and dining room. Thanks to the spacious quarters and to the interesting exhibits it was possible to entertain the entire company in a satisfactory manner.

The concluding paragraph of the extended account given in the *Royal Gazette and Colonist Daily* may be quoted as an indication of the general impression in Bermuda of the new biological station:

And so concluded an event that marks an era in the history of Bermuda. Everyone present was deeply impressed with the importance of the occasion and there is little doubt that the Bermuda Biological Station is destined for a great future. To all those who have contributed to its foundation Bermuda owes a debt of gratitude difficult to express or repay.

E. G. CONKLIN

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

A CONVENIENT PREPARATION FOR THE REMOVAL OF HAIR

THE depilatory, reported in this paper, has been used in my laboratory for some time. At the suggestion of some colleagues, I have decided to describe the method used for preparing it.

This preparation is *very toxic* if injected subcutaneously or intraperitoneally into white rats or guinea-pigs. There, however, are no apparent toxic effects when it is used for removing hair from large areas on laboratory animals.

There are several advantages in using such a hair remover: (1) inexpensiveness, (2) ease of preparation, (3) quick action, (4) a smooth unscarred area which is necessary in certain types of investigation, and (5) the activity does not decrease markedly on standing.

EXPERIMENTAL

The apparatus consists of three one-liter flasks, one one-hole rubber stopper, one two-hole rubber stopper, glass tubing and rubber tubing for connections. The arrangement of the apparatus is shown in Fig. 1.

Hydrogen sulphide is generated by means of ferrous sulphide and hydrochloric acid in flask No. 1. The gas passes through flask No. 2, which is about

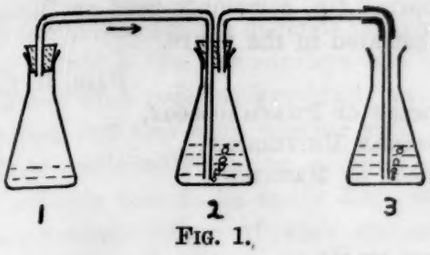

FIG. 1.

one half full of distilled water. Flask No. 3 contains an aqueous solution of barium hydroxide.

The following preparation keeps its activity very well and removes hair in 3 to 5 minutes from areas large enough for ordinary operative purposes. Forty grams of powdered barium hydroxide were added to flask No. 3, which contained 500 cc of distilled water. The contents of the flask were agitated occasionally during several hours in order to insure solution (an electric stirrer would cut down the time and eliminate the shaking). The hydrogen sulphide was generated by treating one ounce of granulated ferrous sulphide with 200 cc of 1–2 hydrochloric acid. The gas was washed by means of the water in flask No. 2 and passed directly into the aqueous solution of the barium hydroxide, which was agitated continuously for the first few minutes and then at intervals of a few minutes. The hydrogen sulphide was allowed to pass into the solution for thirty minutes or until the supply was practically exhausted. The apparatus was disconnected and twenty-five grams of powdered barium hydroxide were gradually added while stirring the solution. After testing the solution for its activity by means of laboratory animals (a small area on one's arm may be used), it was stored in a brown bottle.

The application of the depilatory must be attended with some precaution since the solution will dissolve the nails and cuticle as well as the hair. If one will take 25 to 50 cc of the solution in a beaker and apply it gently by means of a small piece of gauze and a pair of forceps, he will obtain good results. If the coat of hair is heavy, it will be necessary to keep adding fresh solution from the beaker for several minutes. A tongue depressor, spatula or some blunt-edged instrument may be used to gently remove the most of the dissolved hair. The animal is held under a stream of warm water in order to wash off the depilated area and free it from the barium solution. It is best to keep the animal in a warm place until dry and longer if the depilated area be very large.

DISCUSSION

Since the solution was found to be toxic if administered subcutaneously or intraperitoneally, one might expect that it would be injurious if it were used for removing hair. It, however, has been used on dogs, rabbits, guinea-pigs and white rabbits without any apparent deleterious effects.

The depilated areas seemed to grow hair at a slower rate than similar areas which were shaved.

The solution, made according to the method as described above, will retain its activity for several months. The quantity of barium hydroxide treated with hydrogen sulphide and the quantity of barium hydroxide added to the treated solution can be varied somewhat without markedly affecting the activity of the depilatory. One solution, containing approximately the same amount of barium hydroxide as used in the above method, was quite active after standing for one year.

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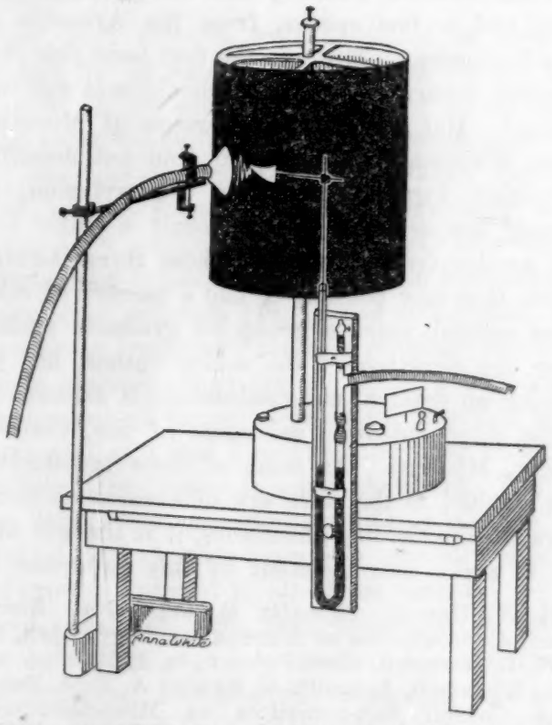
AIR CONTROL OF MANOMETER WRITING POINTS[1]

ARTERIAL and venous pressures are often recorded on a smoked drum by means of a writing point attached to a float. Various devices are used to keep the writing point in contact with the drum in such a manner that a clear tracing will be obtained without so much friction that the recording of pressure changes will be interfered with. This is a simple matter with mercury manometers but when water manometers are used it is often very difficult to obtain a good tracing. A simple method of keeping the writing point against the drum with a delicate but even pressure is shown in the accompanying illustration. A stream of air is blown against the writing point, pressing it against the drum. By regulating the air current the contact of the writing



[1] Demonstrated at the meetings of the Federated Biological Societies at Montreal, 1931.

point with the drum can be very delicately adjusted. The matter of obtaining a uniform pressure over a large range of movement of the pointer is a difficult one on account of air eddies, but a very satisfactory air stream for recording small changes of pressure can be obtained by blowing air through an ordinary fish tail burner tip, commonly used on Bunsen burners, as illustrated in the figure.

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SPECIAL ARTICLES

PRE-CARBONIFEROUS FORAMINIFERA

THE foraminifera, representatives of the most primitive invertebrate phylum, are, paradoxically enough, essentially missing from the roster of the geologically older faunas. The various reasons for their apparent absence can not be discussed here; but because our paleontologic knowledge of the group prior to the mid-Mississippian (when at least one species, *Endothyra baileyi*, definitely became abundant), is so scanty, even the vaguest pre-Carboniferous foraminiferal record is eagerly sought for by paleontologists.

The specimens recorded from the pre-Cambrian of Brittany by Cayeux, however, are regarded by many as unauthentic. Those described by G. F. Mathew from the Cambrian of New Brunswick, according to Howell,[1] turn out to be phosphatic concretions; and a large percentage of the few Cambro-Ordovician and Silurian species of Europe, described by Chapman, Ehrenberg, Brady, Terquem and Keeping, has been looked upon with considerable skepticism by some paleontologists. In 1930, however, Moreman[2] described a rich faunule of arenaceous foraminifera from the Silurian Chimney Hill formation of Oklahoma, and a few species from the Arbuckle and Viola formations. This was the first *bona fide* North American occurrence of pre-Carboniferous age to be reported. But since the appearance of Moreman's paper, Whitcomb[3] has reported (but not described) Ordovician foraminifera from Pennsylvania, and Thomas[4] has described posthumously a single Devonian species from Iowa. To these three American records it is now possible to add a number of others.

For several years a group of graduate students, under the direction of the senior author, has been carrying on detailed micropaleontologic examinations of the older Paleozoic sediments of Ste. Genevieve County, Missouri. As some of these investigations have resulted in the discovery of arenaceous foraminifera in considerable abundance, it is thought advisable to make announcement of this important fact prior to the publishing of the detailed descriptions of the faunas.

Late in 1930 Mr. Dunn found a number of arenaceous foraminifera in siliceous residues of the Bainbridge limestone (Niagaran of southeastern Missouri). Accordingly, on our return to the University of Chicago's Missouri Field camp in June, 1931, we were equipped to carry on microscopical studies of the sediments essentially at their outcrops. In this way it was found that almost all samples of the Bainbridge, regardless of lithologic appearance and stratigraphic or geographic position, contain foraminiferal remains. The Brassfield (Alexandrian) strata of southeastern Missouri also were found to carry an abundant fauna; and later it was discovered that a suit of Brassfield samples, collected by Professor D. J. Fisher near Joliet, Illinois, was likewise replete with the same general types of foraminifera. Furthermore, although the suspected remains thus far found are decidedly fragmental, we have a strong suspicion that the Niagaran beds of the Chicago area and the Waldron strata of Indiana also contain similar forms, though in considerably diminished numbers. So far as the Waldron is concerned we understand that this suspicion is shared by Professor Willard Berry, of Ohio State University. Mr. Dunn will soon have the results of the research on these Silurian sediments ready for publication. In the meantime, however, it may be pointed out that forms similar to if not identical with the following typical Chimney Hill species are common in the Bainbridge fauna:

Ammodiscus excertus, Cushman.
Ammodiscus incertus (d'Orbigny).
Bathysiphon curvus, Moreman.
Colonammina conea, Moreman.
Lagenammina stilla, Moreman.
Lituotuba exserta, Moreman.
Psammosphaera cava, Moreman.
Sorosphaera tricella, Moreman.
Thurammina triangularis, Moreman.

The similarity of these unusual faunas suggests the possibility of accurate correlation of a number of mid-Paleozoic formations on the basis of arenaceous foraminifera. The writers are fully aware of the fact that this is in part a presumption. The effectiveness

[1] B. F. Howell, Appendix E, Rept. Nat. Research Council Sub-committee on Micropaleontology, April, 1931.
[2] W. L. Moreman, *Jour. Paleont.*, iv, 1, 1930.
[3] L. Whitcomb, Appendix E, Exhibit A, Rept. Nat. Research Council Sub-committee on Micropaleontology, April, 1931.
[4] A. O. Thomas, *Jour. Paleont.*, v, 1, 1931.

of such a correlation scheme depends not alone upon the extent to which the foraminifera can be found in beds wherein their presence previously has not even been suspected, but also upon whether such primitive types change sufficiently during a relatively short elapse of geologic time to be really diagnostic. The tremendous geologic range of such species as the modern *Ammodiscus incertus* (d'Orbigny) might seem to indicate that they do not, but on the other hand there is no difficulty in separating Alexandrian Brassfield samples from the Niagaran Bainbridge sediments solely on the basis of differences in their foraminiferal remains. That a number of other mid-Paleozoic formations may yield foraminifera when the unsoluble residues are studied is suggested by the fact that we have also found the Helderbergian Bailey formation of southeastern Missouri to contain their abundant remains, though the assemblage has not yet proven to be particularly diversified. This new fauna is being described by Mr. Hunter.

In addition to the very definite discoveries mentioned above, it may be well to list the less positive and the entirely negative results of our studies. The Oriskanian Little Saline limestone pretty certainly contains these microscopic organisms, although our specimens are thus far both rare and discouragingly fragmentary. The mid-Devonian Grand Tower, Beauvais and St. Laurent formations have been carefully examined, thus far with only negative results. The Ordovician Joachim formation apparently does not contain foraminifera, but there is evidence, still far from certain, that the Plattin, Kimmswick and Fernvale may yet yield unquestionable specimens. A number of older formations of the Ozarks, such as the Gasconade, Jefferson City, Cotter and Powell, also have been studied, but without positive results. As H. S. McQueen, of the Missouri Bureau of Geology and Mines, however, has run literally thousands of careful mineralogical studies on the insoluble residues of these sediments without noticing foraminifera, we may at least assume that very few of the organisms were preserved in these strata.

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TREATMENT OF COTTON ROOT-ROT WITH AMMONIA

A TOXIC effect of ammonia on the root-rot fungus, *Phymatotrichum omnivorum* (Shear) Duggar, has been determined as a result of a series of experiments in growing the fungus in different media, during the season of 1931. When ammonium salts were used as the source of nitrogen in Duggar's solution, the growth of the fungus was notably restricted, and this indication was confirmed by subsequent tests. It was found that both the mycelial and sclerotial stages of the fungus were killed by short exposure to the gas, and where dilute concentrations of the hydroxide were applied under field conditions, the fungus was killed in the tissues of cotton roots.

In the cultural experiments with ammonium nitrate and ammonium sulfate, when used at a concentration to yield approximately 12.4 grams of nitrogen per liter, very little growth of mycelium appeared after intervals of 11, 18 and 31 days, whereas the other sources of nitrogen, *viz.*, calcium nitrate, potassium nitrate and sodium nitrate, produced an abundant growth.

After pH readings had revealed that the suppression of growth with the ammonium salts was not attributable to an acid reaction of the media, the apparent toxic effect was studied. The procedure was as follows: fresh cultures were prepared in 125 cc Erlenmeyer flasks containing 50 cc of neutral carrot agar, and after growth was well established over the media, the mycelium in each of four flasks was exposed for 20 minutes to dilute ammonium hydroxide (.1, .5 and one per cent. by volume, prepared from 28 per cent. ammonia water). Four untreated flask cultures were reserved as checks. After the above exposures, transfers were made immediately from the treated and check flasks to neutral carrot-agar slants in test-tubes. In every case, the inoculum from the cultures treated with ammonium hydroxide failed to grow, but growth occurred in all cases from the check inoculum.

Other flask cultures were subjected to ammonia treatment for 30 seconds by generating the gas from 500 cc of 28 per cent. ammonia water and allowing this to enter the culture flask by means of glass tubing. The gas treatment also completely inhibited growth of the fungus when transfers were made. Further tests were made with root-rot sclerotia by exposing them to ammonia, chlorine and formaldehyde gas for short intervals (10, 15 and 20 seconds) with the ammonia treatments showing complete mortality.

Roots obtained from freshly wilted cotton plants were exposed to the same gases for intervals of 30 seconds and of 1 and 2 minutes and later tested for viability of the fungus in moist chambers. The ammonia treatments prevented growth, while the formaldehyde and chlorine were only partially effective. Good growth occurred in all of the checks.

Field tests have also been conducted at various times during the season to compare the effectiveness of 6 per cent. solutions of ammonium hydroxide, formalin and sodium hypochlorite when applied in the soil to the roots of infected cotton plants. In

these experiments large mature plants were selected and treated with the different disinfectants by saturating the soil about the roots of the plant. After forty hours the plants were removed from the soil and the roots placed in moist chambers. Observations were made at one-day and five-day intervals. Plants treated with ammonium hydroxide showed no growth of mycelium; the plants treated with formalin, slight growth; those treated with sodium hypochlorite, good growth; the checks, good growth.

The disinfection with ammonia appears to be more complete than with other chemicals used in comparative tests, and the danger of killing adjacent plants, as by formalin treatments, is avoided. The possibility of utilizing ammonia or ammonium compounds for the control of the disease in cotton fields as well as for protecting ornamentals or shade trees is suggested, and further experiments are being made.

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PREVENTION OF BLOOD COAGULATION BY CYSTEINE[1]

IN 1928[2] one of the writers reported that when oxidation in Berkefeld filtrates of the Rous chicken tumor was prevented by means of cysteine, the spontaneous loss of infectivity of the filtrates which takes place rather quickly with this virus is very markedly inhibited. At the same time a precipitate which usually forms under aerobic conditions, varying in degree from a slight opalescence to a definite flocculation, no longer occurs, and the filtrates remain water clear for days, as long as they are protected from the air. It was at first believed that this precipitate was in some way connected with the filterable virus, since "infectivity" disappeared more or less parallel with its formation. Control experiments, however, in which Berkefeld filtrates of embryonic chick tissues were prepared, showed this precipitate to an even more marked degree than tumor filtrates, and here, too, its formation was largely or completely prevented by reducing with cysteine.

The precipitate appeared to consist of protein, and the possibility was obvious that it resulted from a slow coagulation of some of the saline soluble tissue proteins and that oxidation played a part in its formation. It was logical to inquire whether oxidation might enter into the chain of events resulting in the coagulation of blood. There has been in the past some evidence to this effect, but as far as we could find from a hasty review of the recent literature, nothing very direct.

It was a simple matter to withdraw a few cc of human blood in an oiled syringe and transfer 1 cc quantities to three test-tubes, each containing two glass beads. One tube served as control, containing 0.5 cc of saline. The second tube contained 0.1 g of cysteine hydrochloride, neutralized with NaOH in 0.5 cc saline, and the third tube 0.1 g of alanine in saline as a control on the NH_2 and COOH portion of the cysteine. The blood in each tube was quickly covered with 2 cm of melted vaseline. By tilting the tubes at intervals, it was possible to observe when coagulation took place by the movement of the glass beads. Tubes 1 and 3 coagulated in about ten minutes. Tube 2 remained completely uncoagulated for twenty-four hours, was then opened, the blood drawn off, and found to be quite fluid, and was discarded. Repetition of the experiment showed that 0.01 g of cysteine hydrochloride was insufficient to prevent the coagulation of 2.0 cc of blood, but that 0.05 g would do so.

Further experiments indicated that thorough aeration of the cysteinized blood, leading to oxidation of the cysteine to cystine, would result after some time in coagulation.

The further analysis of the mechanism of this phenomenon is definitely outside the scope of a bacteriological department, but the experiment can be so easily repeated that it seems worth while to describe it in the hope that by means of it some further light may be shed on the obscure question of blood coagulation.

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[1] From the Department of Bacteriology and Immunology, Harvard University Medical School, Boston.

[2] J. H. Mueller, SCIENCE, 68: 1752, 88, July 27, 1928.

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